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THE

EDINBURGH
MEDICAL AND SURGICAL
JOURNAL:

EXHIBITING

A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

IN

MEDICINE, SURGERY, AND

PHARMACY.

VOLUME TWENTY-NINTH.

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CONTENTS.

PART I.—Original Communications.

PAGE

Art. I. Essay on the Treatment of Caries, with Remarks on the Diseases of Bone in General. By John Inglis Nicol, M. D., - 1

II. Cases and Observations in Medical Jurisprudence. I. Imputation of Poisoning. II. Poison with Arsenic. By Robert Christison, M. D. Professor of Medical Jurisprudence and Police in the University of Edinburgh, &c. &c. - - - - 18

III. On Stricture of the Lachrymal Duct. By Thomas Pridgin Teale, Esq. Surgeon to the Leeds Public Dispensary, - 27

IV. A Case of Elephantiasis Arabum, with remarks. By Robert Hull, Member of the Royal College of Surgeons, - - 31

V. Physiological Experiments and Observations on the cessation of the contractility of the Heart and Muscles in the Asphyxia of Warm-blooded animals. By James Phillips Kay, M. D., - 37

VI. Case of Extra-uterine Fœtation. By John Cooper, Esq. Surgeon, Liverpool, - - - - - - 66

VII. On the Duration of Fatal Cholera. By Mr Edmund Tatham, Surgeon, and Member of the Royal Medical Society of Edinburgh, 70

VIII. On the Causes of the Fatal Termination of certain Cases of Bronchotomy. By William Cullen, M. D. Surgeon to the Royal Infirmary, Lecturer on Anatomy and Physiology, &c. &c. - 75

IX. Case in which Tracheotomy was successfully performed. By Mr William Craigie, Surgeon, Midclova, Aberdeenshire, - 83

X. On the Poisonous Effects of the Mussel, (*Mytilus edulis.*) By James Combe, M. D., - - - - - 86

XI. On the Coup de Soleil. By James Mitchell, Esq. Surgeon, Royal Navy. Communicated by Dr Duncan Junior, - 96

XII. Memoir of the late Dr Robert Jackson, Inspector of Army Hospitals. Read by Dr Borland at the Anniversary Meeting of the Officers of the Medical Department, 12th May 1827, - 110

XIII. Notes of a Case in which the Canal of the Larynx, after being nearly obliterated, was re-established. By Robert Liston, Esq. Surgeon, Lecturer on Surgery, &c. - - - 118

PART II.—Critical Analysis.

Art. I. Lectures on the Operative Surgery of the Eye; or an Historical and Critical Inquiry into the Methods recommended for the Cure of Cataract, for the Formation of an Artificial Pupil, &c. &c. &c. By G. J. Guthrie, Esq. - - - - 119

II. 1. Outlines of Human Physiology. By Herbert Mayo, Surgeon and Lecturer on Anatomy.
2. Laws of Physiology, translated from the Italian of B. Mojon, Professor Emeritus of the University of Genoa. By George R. Skene, Member of the Royal College of Surgeons, London, &c. 141

III. 1. Exposé des Divers Procédés, &c.—Account of the different methods employed to the present time to cure Stone without having recourse to the operation of Lithotomy. By J. Leroy (D'Etiolle,) M. D., Paris.
2. Remarques sur l'Uretre, &c.—Remarks on the Urethra, Male and Female. By M. Amussat.

2367

PAGE

3. De la Lithotritie, &c.—On Lithotrity, or Destruction of Stone within the Urinary Bladder. By Dr CIVIALE.
4. Rapport, &c.—Report presented to the Royal Academy of Sciences, by MM. CHAUSSIER and PERCY, on the method proposed by Dr Civiale for destroying Stone, without having recourse to Lithotomy.
5. Memoire sur la Lithotritie, &c.—Memoir on Lithotrity. By ALPHONSO TAVERNIER, D. M. P. - - - - 150

IV. Transactions of the Medical and Physical Society of Calcutta, Vol. II. 170

V. Versuch einer kritischen Geschichte der bei vaccinirten beobachteten menschenblattern, nebst Untersuchungen über die Natur, die Ursachen, und die Verhütung dieser Krankheit.—Von Dr A. F. LUDERS, &c.
A Critical and Historical Essay on Small-pox, as it occurs after Vaccination, with an inquiry into its Nature, Causes, and Prevention. By Dr ADOLPHUS FREDERICK LUDERS, Physikus of Eckenförde in the Duchy of Silesia, Fellow of the Royal Medical Society of Copenhagen.
De Varioloidibus et Varicellis. Scripsit NIC. CHRISTIANUS MÖHL, &c.
On Chicken-pox and the Varioloid diseases. By N. C. MÖHL, Joint Physikus of Copenhagen, and Fellow of the Royal Medical Society of that city. - - - - - - 178

VI. Memoirs of West Indian Fever; constituting Brief Notices regarding the Treatment, Origin, and Nature of the Disease commonly called Yellow Fever. By JOHN WILSON, M. D. R. N., - 189

VII. The Morbid Anatomy of the Human Brain: being Illustrations of the most frequent and important Organic Diseases to which that viscus is subject. By ROBERT HOOPER, M. D. Batchelor of Physic of the University of Oxford, &c. &c.
2. The Morbid Anatomy of the Brain. By ALEXANDER MONRO, M. D. Professor of Anatomy and Surgery in the University of Edinburgh, and President of the Royal College of Physicians, &c. &. 205

VIII. Essai sur les Tubercules, &c.—An Essay on Tubercles. By HENRY CLERMOND LOMBARD, M. D. Member of the Royal Medical Society of Edinburgh, &c., - - - - - 210

PART III.—MEDICAL INTELLIGENCE.

On the cause of death after the ligature of the eighth pair of nerves, - 214
On the Narcotic Properties of Carbonic Acid Gas, - - - 215
Experiments on the Effects of Bryonine on the Animal System, - ib.
Method of recognizing the stains caused by blood on steel instruments, - 216
East India Senna, - - - - - - - ib.
East India Opium, - - - - - - - ib.
Lack Boxes, - - - - - - - ib.
New Escharotic, - - - - - - - ib.
Case of Hydatids of the Uterus, - - - - 217
On the Treatment of Amenorrhœa and Leucorrhœa, - - ib.
Treatment of Mercurial Salivation, - - - - 218
Sulphate of Quinine given in Large Doses, - - - ib.
Case of Protracted Epilepsy cured by removal of a depressed portion of bone, 219
Spontaneous Emphysema, - - - - - - 220
Case of Ligature of the Carotid for supposed aneurism, proving fatal by laceration of the artery under the ligature, - - - ib.
Employment of Artificial Respiration in the Treatment of Poisoning with Opium, ib.
Urinary Concretion of unusual dimensions, - - - 221
Lithotrity and Stone-Drilling, - - - - - - ib.
Successful Amputation at the Hip-joint, - - - 223
Fatal Hemorrhage from the rupture of a Varicose Vein, - - 224

NOTICES TO CORRESPONDENTS.

Communications have been received from Drs Davy, Ryan, Milligan, Montgomery, and Mr Darbey.

The following works have come to hand:—

A Manual of Comparative Anatomy, translated from the German of J. F. Blumenbach, with Additional Notes. By William Lawrence, Esq. F. R. S. &c. 2d Edition. Revised and Augmented by William Coulson, Demonstrator of Anatomy at the Medical School, Aldergate Street. London, 1827.

An Introduction to the Comparative Anatomy of Animals; compiled with constant reference to physiology, and elucidated by twenty copperplates. By C. G. Carus, Med. et Phil. Doct., &c. &c. Translated from the German by R. T. Gore, Member of the Royal College of Surgeons in London. In Two Volumes, 8vo. London, 1827.

A Practical Treatise on the Diseases of the Skin; arranged with a view to their constitutional causes and local characters, &c. &c. 2d Edition, corrected and enlarged. By Samuel Plumbe, M. R. C. S. &c. &c. London, 1827.

The Lectures of Sir Astley Cooper, Bart. F. R. S. Surgeon to the King, &c. on the Principles and Practice of Surgery; with additional Notes and Cases. By Frederick Tyrrell, Esq. Surgeon to St Thomas's Hospital, &c. &c. Vol. III. London, 1827.

Introductory Lectures to a course of Anatomy, delivered by the late John Barclay, M. D. F. R. S. E. &c. &c.; with a Life of the Author. By George Ballingall, M. D. Regius Professor of Military Surgery in the University of Edinburgh. Edinburgh, 1827.

A Manual of Midwifery for the use of young Practitioners of both sexes. By William Maclure, Surgeon, Licentiate of the Faculty of Physicians and Surgeons, Glasgow, &c. &c. London, 1827.

No. I. of Flora Medica; containing Botanical Descriptions, Natural History, Chemical Properties and Analyses, Medical Properties and Uses, &c. &c. of the different species of Officinal Plants comprised in the latest editions of the London, Edinburgh, and Dublin Pharmacopœias. A list of the Indigenous Plants possessing medical properties not included therein; a list of Poisonous Plants, &c. &c.; illustrated with coloured delineations. Edited by a Member of the London College of Physicians, F. L. S., and assisted by several Members of a Botanical Society. London, Published by Callow and Wilson, Prince's Street, Soho, November 1, 1827.

Thoughts on Vaccination, and the cause of its failing to afford the same protection against Variola as formerly. By John M'Ghie, Surgeon, R. A. &c. &c. 2d Edition, enlarged. Dumfries, 1827.

NOTICES TO CORRESPONDENTS.

An attempt to establish a New System of Medical Education, with a view to suppress ignorance, and preserve the respectability and utility of the Medical Character. By John Charles Litchfield, Esq. &c. &c.

Journal des Progres des Sciences et Institutions Medicales en Europe et en Amerique, &c. par une Association de Medecins. IVme volume. Paris, 1827.

The Medico-Chirurgical Review. Conducted by Associated Physicians and Surgeons, and superintended by James Johnston, M. D. No. 13. July, 1827.

The Philadelphia Journal of Medical and Physical Sciences, &c. Edited by N. Chapman, M. D. W. P. Dewees, M. D. John Godman, M. M. and Isaac Hays, M. D. New Series, No. 10.

Observations on Complaints of the Stomach, &c. &c. By George Uwins, M. D. London, 1827.

Ragionamento sulle Forze effettrici e coadjutrici della circulazione della sangue, e specialmente su' di novelle forze della medesima associate alla di gia' conosciute, e su' di altri questioni e notizie relative a dette forze e loro effetti tanto nello Stato Physiologico che Patologico, esaminate, e stabilite fino dall' Anno 1808 ed ora con aggiunte, note, e schiarimenti in fine: Date in luce dal Dottor Guiseppe Rigaccini di Montalcino, Medico ed autore non che di altri, del Sistema Animale-Organico-Chimico. Roma, 1824. 8vo.

ERRATUM.

In Volume xxviii. p. 418, fourth line from the top, *for* Dr Barry *read* Dr Arnott.

THE

EDINBURGH

MEDICAL AND SURGICAL JOURNAL.

1. JANUARY 1828.

PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*Essay on the Treatment of Caries, with Remarks on the Diseases of Bone in General.* By John Inglis Nicol, M. D. Member of the Royal College of Surgeons, London, Member of the Faculty of Medicine of Tubingen, and one of the Surgeons to the Northern Infirmary at Inverness, &c. &c.

In the early or acute stages of the simple forms of diseases of the bones, enlargement of the vessels, and the other marks of inflammation, are conspicuous characters. The periosteum commonly thickens, and is not unfrequently accused as the origin of the mischief. Suppuration follows; the matter works its way among the soft parts towards the surface, and is discharged by one or more openings, which sooner or later become fistulous. This discharge continues perhaps for years, longer or shorter, according to the situation and extent of the disease. The constitution, if not previously impaired, ultimately gives way. Much of the bone dies; and unless the powers of nature, or the strength of the constitution, aided by the usual remedies resorted to in such cases, be sufficient to throw it off; or if manual assistance be not successfully interposed, hectic comes on, and gradual exhaustion follows.

In the more complex diseases of the bones, or those of the osteo-sarcomatous character, the structure becomes altered by inflammation sometimes, though not always, of specific origin.

Enlargement takes place, and the osseous substance degenerates into a mass, somewhat analogous to cancer,* being usually preceded by similar lancinating pains, which become more acute or severe, as the disease advances. This is sometimes rapid, sometimes slow. In its progress the periosteum, ligament, muscle, tendon, vessel, nerve, in short every thing it approaches, become involved in the diseased action. All become a homogeneous mass, which is sometimes hard, sometimes soft, or often both textures are to be found existing together. The skin ulcerates, and the uncovered tumefaction exhibits an irregular, cancerous surface. As hectic advances, the strength gradually diminishes, and the patient dies. In some cases, however, the morbid enlargement is confined entirely to the bone itself, the osseous growth simply by pressure causing the absorption of the soft parts, which impede its progress, and the bones themselves are equally compelled to give way under similar circumstances. This is well exemplified in their absorption from the pressure of large aneurismal tumours. All the bones are liable to these malignant affections ; but they are found to occur most frequently in the bones of the face. Tumours of the antrum are very common ; and whether the complaint originates in the bone or investing membranes, the former invariably participates in the morbid action as the disease advances.

Scrofula, sibbens, syphilis, as well as several other diseases of the whole system, produce considerable changes in the bones. In a case of tubercular sarcoma which fell under my observation, the bones of the skull were studded, both on the inside and outside, with grayish pulpy patches, having osseous radii shooting up through them, the tables being entirely absorbed, and the diploe only left in the parts thus affected. Diabetes mellitus in its latter stages produces a remarkable change in the alveolary processes. They become carious, the gums spongy, and the teeth fall out.† Several such cases have come under my notice.

The general affections which more particularly refer to the bones themselves may be comprised under the terms *Mollities*, *Rachitis*, *Fragilitas*, and *Exostosis*. But as these diseases are, comparatively speaking, of rare occurrence, and little under our control, I shall for the present confine my observations to those in which our art can be interposed with advantage.

---

* In this country, as well as in other places, most tubercular formations of malignant character are usually designated by the appellation of cancerous.

† This is not a uniform result. When it takes place, the process seems to be one of absorption, in which the vessels of the lining membrane gradually destroy the connection between the alveolus and the tooth, while the alveolus itself is successively wasted or consumed.—Ed.

Nodes or *Tophi*, *Caries*, and *Necrosis*, are the affections which most frequently come under the management of the surgeon. These may result either from injury done to the parts, or from a morbid condition of the system; and where there is no evidence of the one, it is legitimate to conclude that they originate in the other.

In the earlier stage of their syphilitic affections, they are usually overcome by general treatment alone; not so when ulceration has taken place. The constitution already impaired, becomes more so under the continuance of local irritation; and not unfrequently resists every remedy or application which the healing art can devise. But perhaps still greater inveteracy exists in those of scrofulous character; and the practitioner, disappointed in his attempts to remove the former, is often disposed to attribute his want of success to the taint of the latter. Most of the cases of caries or necrosis which I have seen, whether preceded or not, were always accompanied by morbid irritation of the constitution, even where the health might be considered good, and these affections had become habitual. There was an evident languid peculiarity of expression, which conveyed to the mind an impression of the existence of this irritation, and which certainly does not always depend on scrofulous diathesis, as it is often found to disappear almost immediately on the removal of the part offending.

Those affections, which may be considered as purely scrofulous, sometimes appear to originate in the bone; but several other textures remotely situate are attacked successively with similar enlargement and ulceration. And it is highly probable that a like disposition is generated in the system, by the long-continued irritation of an ulcerous or necrosed bone, resulting from simple injury.

Cases of this kind are very common among the poorer classes of society. They seldom seek assistance under fluctuating expectations of amendment. They put off from day to day, until their complaints have resisted the powers of the constitution. In these circumstances the discharge is sometimes less, sometimes greater, varying with every change in the part, which either accidental circumstances or the progress of the disease may produce. The variety of parts affected, their situation and extent, may all preclude much expectation from local management in this state. General remedies alone are relied on, but hitherto with very limited advantage. Nature, however, interposes her influence in some cases deemed utterly hopeless; and when the resources of art have been in vain exhausted, her efforts alone apparently effect a recovery. These cases occur but seldom, and the difficulty with which such ulcerations are found to heal, or the dead parts to be thrown off, are enough to limit our expectations from the powers of nature alone. For although in a few cases the healing process may go on steadily enough, yet no surgeon of experience can justify himself in an entire reliance on a spontaneous cure.

The treatment of such cases has hitherto been but little influenced by the investigation of the causes from which their several varieties have been supposed to originate, or by knowledge of the structures which they had assumed. For ages past the character of the remedies have been pretty much the same; and it is to be regretted that the changes which these diseases undergo have been more successfully examined than the means of averting them pointed out. Observation of alleged causes and minute details of structures, with the exception of those of syphilitic origin, seem thus of little importance in regulating their management in the diseased state. If the affection be presumed to be constitutional, the constitution is attacked; if considered local, local remedies are used; and every effort is made by manual interference to remove it entirely, if at all practicable. The great bulk of failures, however, appear to have arisen from extreme reliance on the one, while the other was indifferently attended to, or neglected altogether. Both seem indispensably necessary, and I feel the more confirmed in this opinion from the want of success in the earlier part of my own practice; while, by the subsequent happy combination which I am about to describe, and which I have now been using for upwards of fourteen years, cures have been effected under the most unpromising circumstances.

Reflecting on the rapidity with which syphilitic affections of the bones give way under the use of mercury and sarsaparilla, I was induced to try their effects on osseous diseases of different origin. But it was not until I had combined active local treatment with the constitutional management that I had the satisfaction of realizing my expectations. The topical remedy which I have hitherto found most successful is a rod or pencil of lunar caustic, conveyed in every case to the surface of the diseased bone.

The cases which I am about to relate, are offered to the profession without the least pretensions to novelty, in so far as regards the remedies employed. The success of their application alone has a claim on its attention. And unless I may be considered to have fallen into a common error of attributing to them that which has been exclusively the work of nature, I am not without hope that the same mode of management may be found equally successful in the hands of others,—a hope which I rejoice to say has, in some measure, been confirmed by the communications of several professional friends who have tried it, on my recommendation, with similar success.

Case I.—A gentleman, a native of Scotland, while leaping a ditch in the West Indies, sprained his right foot. The usual remedies were applied and he got apparently well. Shortly after he perceived a slight tumefaction over the upper and inner surface of the metatarsal bone of the great toe. He went about as usual, occasionally submitting to the leeching; embrocations, and blistering prescribed by his surgeon, neither of which were productive of the slightest benefit. The tumour increased, and he walked lamely. Becoming alarmed and anxious, he requested further assistance, and a consultation was held; the result of which was a recommendation to return to his native country and have his limb amputated. He embraced the earliest opportunity of carrying the first part of this advice into effect. On his arrival he placed himself under my care. I found a tumour, of rather diffuse character and irregular shape, about the size of a large fig, over the middle part of the metatarsal bone of the great toe. Part of it was firm and part elastic, conveying the impression of both solid and liquid contents. It was always increased by walking. Was not painful on pressure, and the integument was of the natural colour. He walked with difficulty, always putting the side of the foot to the ground. It had continued pretty much in the same state for several months. His general health was good.

I applied a bandage, and in a few days the tumour was so much reduced as to enable me to imbed my finger in a deep sulcus in the bone, thus leaving no doubt of its being the seat of the disease. The bandage was discontinued, and the tumour soon acquired its former size. A trocar was introduced into the softer part, and about a table spoonful of a thin brick-coloured matter, with small whitish flocculi, similar to what we see discharged in evacuating suppurated scrofulous glands of some standing, came away. To give free vent to this the opening was enlarged with the knife, and a fungated lobular growth was discovered sprouting from the corroded bone. It seemed weakly organized, some of its limbs having a semitransparent ash-coloured gelatinous appearance. The contiguous bone was so soft as readily to admit the probe to pass through it. Excision of the parts was proposed but resisted. In this state of things I passed *a rod of lunar caustic* of the usual size into the opening, in the direction of the fungus, touching the diseased bone at the same time, and confined this application *to one particular point.* This was repeated daily in the same spot, twirling the caustic between thumb and finger two or three times at each introduction. In a few weeks the tumour gradually disappeared on the inside. A part of it however could be distinctly felt, extending to the contiguous metatarsal bone. A small incision was made above, towards its outside, and the caustic was introduced here also to the depth of half an inch. Violent inflammation of the whole foot succeeded, but was subdued in the course of a few days under the use of antimonials, aided by fomentations with infusion of poppy capsules. The caustic was again introduced as at first, and no inflammation followed. On several occasions, however, I attempted a more free use of the escharotic, with the view of rapidly destroying the morbid growth; but the inflammation which succeeded each of these trials was so violent that I was obliged to desist, and confine its use to the manner already detailed. The fungus was observed gradually to diminish, but the general health became affected. One pound of compound decoction of sarsaparilla was now given daily. The foot was fomented over the part affected morning and evening, and the caustic applications continued as formerly. In a few weeks not a vestige of the fungus could be discerned. The last made opening was permitted to close. The discharge from the other became more healthy, and the parts firm. The small opening at which the caustic was first introduced filled up, and latterly it could not be inserted without some violence. The vigorous process of nature at length resisted it entirely; the parts healed up, leaving a deep furrow from the absorption of the bone; the perfect use of the limb was restored; and during the whole curative process not a particle of bone was observed to escape among the discharges.

Case II.—A young gentleman, about 16 years of age, having over-exerted himself in running, was seized with inflammation along the right shin, extending towards the knee. The surgeon who attended leeched and applied evaporating sedative lotions, by which the inflammatory process was in a great measure subdued. Ulceration however followed some time after along the flat upper part of the tibia; and the integuments gave way *in two* places, about the distance of an inch and a half from each other. The discharge became limited and unhealthy; the upper end of the tibia enlarged; the knee excessively pained; and the general health bad. Leeches and evaporating lotions were again applied over the knee without any benefit, and diaphoretics were also freely administered without any relief. The case becoming alarmingly retrograde, and the parents anxious, I was called in. The knee was much swollen, and the leech-bites had become vesicated, presenting the appearance of pemphigus. The whole upper end of the tibia, and for some inches downwards, was evidently enlarged; its anterior tubercle projected unusually, and was soft, but not tender to the touch. The orifices of the ulcerations previously described had enlarged to nearly an inch in diameter, communicating freely with each other, and exposing the periosteum, which was glairy, and discharged a thin sero-gelatinous rather than sero-purulent matter. Hectic had existed for some time, and the strength was much exhausted. The tibia was now pronounced in a state of extensive disease internally, and the treatment completely reversed. An incision with a scalpel was made on the outside of the enlarged tuberosity, and a rod of caustic introduced close to the bone. One single point of the periosteum at each of the ulcerations below was touched in a similar manner. Fomentations of poppy capsule were applied two or three times daily; four ounces of *Decoct. Sarsaparillæ comp.* were given morning, noon, and night; and from half a grain to a grain of opium exhibited two hours after each dose of the decoction.

In twenty-four hours tranquillity was restored to the limb. In three days the hectic fell off. This plan of treatment was continued, substituting for the opium as many tea-spoonfuls of the following mixture as the stomach could bear without sickness or nausea.

℞ *Antim. tartarizat. gr.* iv. *Solv. in aq. cinnam.* ℥viij. *Tinc. Opii* ʒi. *M.*

The swelling in the knee disappeared; that in the bone diminished; and two thin circular films, evidently separations from the outer lamina of the tibia, were discharged at the openings. The ulcers turned healthy; but as they healed, absorption of the bone underneath became evident, especially at and about the points to which the caustic was applied. The opening at the tuberosity was permitted to heal up, but the caustic was steadily continued at those below. A deep longitudinal rugged sulcus remained in the bone, and the caustic, which now could not be introduced without violence, was abandoned. A very small oozing of matter was observed occasionally at the deeper pits in the furrow, where the escharotic had been applied; but its subsequent use prevented further mischief. In about four months from the time I first saw him he was perfectly whole, and is now a graduate in the profession.

Case III.—An officer in the British army, while serving in the peninsular war, received a gun-shot wound in the leg. The ball passed through the right tibia, high up, and lodged in the inner splintered fibular side of that bone. A probe could be passed through the wound directly to the ball. I proposed its extraction, but the army surgeons, and Sir Astley Cooper, to whom the case had been submitted, having expressed a different opinion, my proposal was not entertained. He walked lamely, and occasionally suffered great pain, which was relieved by immersion in tepid water, low diet, and purgatives. For several years he went on in this way; the ball shifted its position, and the discharge, which always continued more or less, sometimes nearly dried up, but was immediately succeeded by severe inflammation and increase. On one of these occasions the matter was discharged lower down, and the upper orifice healed up. At this time the relief from the topical applications was indifferent, and very tardily procured. Constitutional irritation came on; his general health declined; and numerous external and internal remedies were used without much advantage. Being strongly advised to try the effects of a mineral (sulphuretted) water in this neighbourhood, I again saw him on his way thither. His general health had evidently suffered greatly, and although there was nothing very remarkable in the appearance of the affected limb beyond what has already been detailed, he described the torture which he occasionally experienced, as being peculiarly distressing. I recommended the use of the caustic, and fomentations, with the decoction of sarsaparilla. It was some time after he had commenced drinking the mineral water, and discovering its inefficacy, that he was prevailed upon to adopt them.

The caustic was introduced at the fistulous opening to the depth of a quarter of an inch only, and daily repeated. Fomenting before each application, and compound decoction of sarsaparilla to the extent of six ounces was taken morning, noon, and night.

The relief was immediate; the fomentations and sarsaparilla were soon given up, but the caustic has been continued with comfort ever since. He now walks with little lameness, but the orifice still discharges some matter, and probably may continue to do so while the ball remains in his leg. His general health is good, and he considers himself comfortable from his knowing the means of relief, should he require them.

Case IV.—A young man, a carpenter, falling through the scaffolding of a house, received a contusion on the right leg, a few inches above the outer malleolus, not so severe, however, as to prevent his resuming his work.

About a month thereafter the parts inflamed and swelled. Suppuration followed, but six months elapsed before any outward discharge took place. A sluggish ill-conditioned ulcer was found, which soon assumed a progressive character, the upper extremity cicatrizing, and the lower extending farther down towards the ankle. Various applications were tried, but the ulcer was not healed.

The formation of an abscess over the instep became evident, and the matter discharged itself through several openings, which soon became fistulous. In this state he was admitted into the Northern Infirmary. The foot was œdematous; the fistula discharged ill-conditioned and highly offensive matter; the ankle-joint seemed free from disease, and the general health was still good. The outer malleolus and tarsal bones were discovered by means of the probe to be in a carious state. The foot was ordered to be fomented three times in the day; a pencil of caustic to be introduced at each of the fistulous openings daily; and he was placed upon full diet. Under this treatment some of the sinous openings healed up, but others broke out, and he continued pretty much in the same way for several months, when his health being good, and the foot looking tolerably well, being kept improving under the steady use of the caustic, he was recommended to go home for change of air, and to manage exactly as he had been treated in the Infirmary.

Inattentive to the injunctions given him, his foot got worse, and he returned in about four months thereafter to the Infirmary. His general health was now bad; his foot had an ugly matted appearance, and was increased to double the natural size; numerous fistulous openings occupied the upper part, leading to the carious tarsal and metatarsal bones. There was little motion in the ankle-joint, every attempt at which was accompanied with excessive pain. The caustic and fomentations were resumed; turnip and effervescing poultices were applied; opium and full diet were given; but in despite of every thing the enormous diseased mass seemed to get the better of his constitution. A consultation was now held, and amputation pronounced necessary to save his life. Upon repairing to the ward to make the communication, symptoms of amendment were evident; the openings had become clean, and the ulcerations looked more healthy; the system was less irritable, and the operation was for a time superseded.

No alteration was made in the treatment; the caustic being regularly and steadily continued, together with the fomentations, and the occasional use of poultices, the latter being only applied to such parts as exhibited marks of inflammation. The fistulous openings closed up, one after another, his health continued steadily to improve, and in about three months he was able to walk a little. He left the hospital with one or two of the openings still discharging; but by continuing the same treatment he very soon got perfectly well, and able to resume his work. A few small pieces of bone were observed to come away during the whole progress of his complaint; but the absorption of the bones affected was very considerable. I had the satisfaction of seeing this man a few days ago after walking a distance of ten miles. I examined his foot very particularly. It was much smaller than the other, and the seat of the disease presented an extraordinary appearance of elevations and hollows from absorbed bone, which had never been regenerated.

Case V.—A military gentleman while shutting with his left hand a door, his back turned towards it at the time, felt something snap at the left elbow joint. In half an hour after the most excruciating pain supervened immediately above the inner condyle of the humerus. There was no swelling nor discoloration of the skin. A bandage was applied, which was kept constantly wetted with a solution of acetated ammonia and laudanum. The pain subsided, and nothing further was observed. In a few weeks he joined his regiment, but shortly after was attacked with the most severe rheumatic pains in the head, legs, and arms, and especially in the seat of his former pain at the left elbow-joint. For these symptoms he was subjected to courses of sarsaparilla with mercury; drank chalybeate water; and had many other things given him with intermitting benefit. The elbow-joint became stiff, and was repeatedly blistered, without any abatement of pain. In this way he was afflicted for nearly two years. Having obtained leave of absence, he returned home. The frontal bone over the left eyebrow was now much thickened, and tender to the touch. The lower end of the left humerus was enlarged about the size of the fist. The ends of the fore-arm bones appeared healthy, but very little motion of the joint was admissible, from the obliteration of the humeral fossæ, by the altered structure of the bone; which, by his own account, was unquestionably making progress, and had attained a considerable size before it was discovered by the regimental surgeon. The health was bad, and the rheumatic pains very distressing.

He was ordered four ounces of compound decoction of sarsaparilla morning, noon, and night; and as many tea-spoonfuls as the stomach could bear of an antimonial mixture, similar to that prescribed in case second, two hours after each dose of the decoction. A small incision was made with a scalpel, in the most prominent part of each condyle of the diseased humerus down to the bone; and a pencil of lunar caustic being introduced into the opening, was turned round two or three times between thumb and finger, pressing at the same time against the bone. This application was repeated daily, and the joint fomented prior to each dressing. The diet was light and nourishing. The pain in the joint gradually diminished; the enlargement of the frontal bone disappeared, and his health improved rapidly. This plan of treatment was continued for some time, when he was again ordered to join his regiment. Passing through Edinburgh he consulted an eminent professor, and an equally distinguished staff-surgeon, then at the head of the army medical department in Scotland, both of whom strongly urged him to desist from moving the joint, and to discontinue the caustic, that anchylosis might take place. I as strongly urged my own mode of treatment, which was continued; and I have the satisfaction of knowing, that, although the morbid conformation of the humerus was not removed, its progress was completely arrested; the limited motions in the joint were preserved, and his health perfectly restored.

Case VI.—A boy about eight years of age was attacked with scarlatina, and soon after his recovery he was seized with one of those violent inflammations and suppuration of the internal ear which so often follow this disease. The purulent discharge continued for several weeks, and his hearing became impaired. The surgeon who attended used astringent injections, but latterly directed the parents merely to keep the parts clean with tepid water. The discharge becoming more copious, exceedingly offensive, and the deafness increasing, I was consulted. On examination I discovered a fungus obliterating the whole passage at its innermost extremity; and the character of the discharge was such as left no doubt as to the state of the bone. Three ounces of compound decoction of sarsaparilla was given, morning, noon, and night; a tea-spoonful of the antimonial mixture with opium half an hour after each dose of the decoction; and one grain of Plummer's pill every night at bed-time. A small pencil of lunar caustic was inserted into the fungus, and regularly repeated every day at the same point; a fomentation being applied over the ear previous to each application of the caustic. The parents were instructed to manage these applications, and he returned home.

In a few weeks the fungus disappeared entirely, and a piece of bone, about three-fourths of an inch long, and one-fourth in diameter, was discharged at the meatus; his hearing was perfectly restored, and the discharge dried up.

Case VII.—A lady rising suddenly from a trunk in which she had been packing clothes, twisted her right knee so severely, as to render her incapable of moving.

Leeches, evaporating lotions and foments, were applied successively, with considerable benefit; but she could not move the limb without exquisite pain in the whole knee-joint, and was thus compelled to keep the extremity constantly in the horizontal position.

Pain, with a burning sensation shooting up from the lower end of the tibia towards the knee, followed, and seemed to be more concentrated at the upper end of the bone, about three-fourths of an inch laterally and inwardly from the anterior tuberosity, which felt tender to the touch. The joint was neither swollen nor outwardly inflamed. Blisters and fomentations were applied without benefit.

Conceiving the case to be one in which the articular cartilages were more implicated than the bones, I strongly urged the use of caustic. Five months of great suffering however elapsed before she could be prevailed on to try it. It was only when even opium ceased to afford the accustomed temporary relief that she yielded to my recommendation. At this time there was no change of parts worthy of remark beyond what has been stated.

An incision with a scalpel was made into the severely pained, though slightly elevated part of the tibia, and a large pencil of caustic introduced to the bone. Fomentations were also used, and the caustic daily inserted, and turned two or three times round in the opening. Soon after the purulent discharge commenced she experienced manifest relief; but when the caustic was by any chance intermitted, the pain immediately returned, and as immediately disappeared on its reapplication. Tiring however of its use, she allowed the issue to heal, but was soon compelled to have it again re-established. By its continuance she got so well as to be able to put her foot to the ground, and with the assistance of a crutch to walk a little. The joint was stiff (not anchylosed) but free from pain.

The issue was again allowed to heal up; and she went on daily improving for about two months, when unfortunately, in reaching for something, she lost her balance, fell backward, and twisted the affected knee. This was followed with severe pain, inflammation, and swelling of the whole joint, which was subdued by repeated leeching, fomentations, and by the insertion of the caustic issue on each side of the *ligamentum patellæ*. She gradually got so much better as to be able to resume the use of the crutch, putting the toes only to the ground. The limb being contracted by long continuance in one position precluded any greater use of it. Her general health was tolerably good throughout the whole progress of her complaint, and she was now so far recovered as to be able to walk tolerably well with the assistance of a stick.

To the preceding cases, numerous others, some of them equally unpromising, might be added, in which precisely the same mode of treatment was resorted to with similar success. It was only where the system was completely undermined by long-continued purulent discharges from extensive disease, in the more advanced stages of those of specific character, in old hip and knee-joint cases, and in carious affections of the bodies of the vertebræ, that either the local or constitutional remedies proved of little use. But in most other cases, from the judicious application of the *argentum nitratum*, with the occasional use of the sarsaparilla, both of which constitute the principal features of the treatment now recommended, the benefits experienced were as decided as could have been desired.

It is indeed true that where the diseased or dead portion of a bone can be readily and easily removed, the cure is expedited by seasonable operation. But this cannot at all times be effected, both from the situation and extent of the parts on the one hand, and the timidity of the patient on the other. Besides, there are many excellent surgeons in the profession, who frequently meet with cases favourable for such measures; but not possessing the necessary dexterity in surgical manipulations, prefer milder measures, or letting them alone, to attempts at requisite but difficult operations. Even were it otherwise, there are some patients who would infinitely sooner carry on after their own way, and prefer continuing the victims of disease rather than submit to cutting, sawing, or chiselling, although assured that ten minutes, or as many seconds only, thus occupied, would afford substantial and permanent relief.

It is under these circumstances that I consider milder treatment, if at all successful, of some value. That which I now with deference attempt to advocate having in the course of my own experience proved most satisfactory, I feel myself warranted in asserting, that although these means may be ofttimes slow in effecting the desired change, yet in those cases in which the constitution is at all susceptible of improvement by the local interruption of the diseased action they will ultimately be sure.

Within the limits of an essay such as the present it is impossible to be so minute in detail as could be desired; but the simplicity of remedies admitting of such general application will in some measure apologize for those omissions, which more complex management would render indispensable. Considering the manner of conducting the treatment at present of most importance, I shall now proceed to make a few general observations on the individual remedies usually resorted to, and endeavour to point out such peculiarities in their application as may be deemed of greatest value in a practical point of view.

General blood-letting can only be resorted to with advantage in the acute stages of those diseases of the bones, which partake of the gouty or rheumatic character; and even here only where the system is robust or plethoric. Under almost every other circumstance I consider it entirely inadmissible; since any temporary benefit will be found to prove no compensation for the aggravation of the morbid irritability which often precedes or almost invariably follows these affections.

Measures less fraught with injury to the general strength seem to promise more substantial advantage. In the earlier part of my practice I was in the habit of giving the compound powder of ipecacuan, solutions of tartarized antimony with laudanum, or opium with calomel. Latterly I have used with still greater advantage the *Vinum Colchici*, with Battley's *liquor opii sedativus.* These I have used in the proportions of two or three parts of the former to one of the latter; giving to an adult thirty drops or more every three or four hours, according to the urgency of the symptoms, and continuing until the stomach or bowels became affected, or copious perspiration was excited.

Of the *colchicum* I cannot speak in too high terms; and must say that the inflammatory action will be extremely rapid, or have done much mischief previous to its use, if this medicine does not arrest its progress. As it often leaves an indescribable languor of the whole system for thirty or forty hours after it has taken effect, it must be intermitted until the state of collapse has entirely disappeared. To leeching it will be found an adjunct of the greatest value. Whether the affection be superficial or deep-seated, if there be much pain, or reason to apprehend acute action, leeches should certainly be applied over or as near the part as possible. Scarifying and cupping, from the greater irritation, may be more beneficial in less active cases. But I have remarked, that in almost every instance when the most decided relief did not follow local abstractions of blood, proportioned to the magnitude of the mischief, especially by leeching, either simple ulceration, necrosis, or disease of a specific character, soon become manifest.

After the acute stages of inflammation have passed away, by ordinary tact, the periosteum will often be felt thickened, or the bone itself enlarged. In this state it is customary to resort to embrocations, liniments, and blisters. Although the two former are of considerable use in reducing enlargements of the soft parts, under similar circumstances, they are unquestionably not productive of so much good here. Blistering, and keeping a part open with *ceratum sabinæ*, I have found to answer much better, and still more so the use of the ointment of tartarized antimony. The purulent eruption produced by this application I have often seen to exert a powerful influence over the diseased part; and it combines the further advantage of being more easily borne.

In Germany, affections of the bones and joints, especially knee-cases, are believed to be the consequences of repelled cutaneous eruptions; and the particular affection of the former, it is alleged, may be identified with the peculiar character of the latter. * In Britain, an idea nearly similar has been promulgated regarding syphilitic diseases, where it is asserted that several varieties of secondary eruptions can be identified with a corresponding variety in the primary sore.† Neither of these opinions, however, have obtained sufficient weight among the profession to influence the general practice; although, if once confirmed, in every view they would be of the highest importance, as we could produce almost every variety of cutaneous affection, although divested of their specific characters, by artificial means. Thus the erythematous inflammation may be caused by stinging with nettles; the papular by ammoniacal embrocations; the vesicular by cantharides; the pustular by tartarized antimony; and the most intense itching by the *Dolichos pruriens*, and so on.

In many cases, after the disease has been arrested by the means to which I have adverted, the bone continues enlarged, or the periosteum thickened, and the application of a soap or mercurial plaster, with slight compression by means of a bandage, where it can be applied, seems all that is necessary. But often absorption does not take place, and the enlargement may continue, although perfectly harmless, during the remainder of life.

Where, however, the mischief is still disposed to show itself, from slight causes or exertions, the establishment of a caustic issue will be indispensably necessary. When the most prominent part can be easily reached, I have invariably made an incision with the point of a scalpel, down to the bone, and not larger than was sufficient to admit a pointed rod of lunar caustic, of the usual size, which was immediately introduced, twirled round two or three times, and instantly withdrawn. A poultice of wheaten bread and milk, or mashed turnips, was then applied, and renewed twice a-day until the eschar separated, or the purulent discharge was established. The caustic was introduced in a similar manner, daily or every second day, according as the slough was thrown off. When, however, the diseased structure could not be reached without injury to important soft parts, I contented myself with an incision to the depth of one-fourth or one-half inch, and using the caustic as already mentioned.

In hip cases and vertebral affections I have used the caustic potass exactly in the same way, and certainly with much greater satisfaction than the usual slow process of destroying the integuments through an aperture cut in sticking-plaster. In either case it is not necessary, after the manner here recommended, to wet the escharotic, the blood being abundantly sufficient to dissolve as much as will be enough to answer every purpose; while its viscid and coagulating property will prevent unnecessary destruction of parts. The potass, however, will not admit of such free use as the other, and should be almost immediately withdrawn after its introduction.

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* This opinion has been entertained and acted upon for a long period by Professor Autenreith senior; and the success attending his practice affords the strongest proof of its being well founded.

† Vide Carmichael on Syphilis. Quarto edition.

Short of the actual excision of the parts, I consider the lunar caustic to be the sheet-anchor in the treatment of affections of the bones. In the hands of all practitioners it has proved of the highest value, but, like every other remedy of established reputation, is sometimes liable to abuse. If too liberally applied, or too often repeated, severe inflammation will inevitably follow; an irritation intended to be contiguous or remote will become continuous. But if confined to one point, or where fistulous outlets have been established, or if it is introduced to a very limited depth into one or more of them, no mischief need be apprehended; on the contrary, the inflammatory tendency will be subverted. I have observed this in a remarkable manner confirmed in a case of caries of the orbital ridges of the superior maxillary bones, when the eyes were exceedingly prone to inflammation. A few hours after the introduction of the lunar caustic, (reduced to the size of a crow quill,) this would entirely disappear, but was always sure to return, if by any chance it happened to be intermitted, and was invariably aggravated if too freely used. The same thing I have often observed in caries of the bones of the hand and foot, and was consequently obliged to have the caustic cast into rods of much smaller dimensions than usual,—accommodated both to the size of the opening, and the extent of the affection. Where the carious or necrosed bone could be readily reached, I usually passed the caustic to it; but where the soft parts intervening were greater than half-an-inch in thickness, I never penetrated deeper; and the greater the depth, the less free use in turning was observed, at all events until the parts became accustomed to its application.

When inflammation supervenes in cases of caries or necrosis, either from the too free use of the caustic or other causes, the application of fomentations, with decoction of poppy capsules, will be found among the most tranquillizing. At all times, indeed, whether inflammation exists, or threatens, or not, I have found fomentations of great advantage. In those cases where the action of the parts have become languid from long-continued discharges, and the constitution is weakened, the local energies will be considerably improved by fomentations; and they are further attended with another advantage of no mean importance, viz. that of keeping the parts always clean. Under these impressions, I have continued to use fomentations for a few minutes or longer, before each application of the caustic; and I may observe, that I have not found the same advantage from immersions into, or pouring the tepid infusion or decoction over the parts, as from wringing flannel cloths dipt into, and applying them as hot as the patient could comfortably bear.

The occasional use of turnip poultices will be found of some service, but unless frequently renewed, they emit a disagreeable fetor, both from the change which soon takes place in themselves, as well as admixture with the offensive discharges. It will, therefore, be advisable to dust them over with powdered charcoal, previous to their application, which should always be preceded by fomentation.

When the parts have acquired a healthy character, one fistula will heal up after another; and their vigorous action will resist the further introduction of the caustic; but so long as it can be used without actual violence, it will certainly be prudent to protract or continue to resist final cicatrization.

It is perfectly true that no effect will be produced by the application of caustic to inert or dead osseous matter. But the irritation of the contiguous vessels will excite absorption of the offending parts, or cause the living to throw off the dead; and while this irritation is confined to particular points, the influence over the disease will be exerted in the direction of the exterior irritant, proceeding from the remotest part of the interior.* But in whatever way the caustic acts, whether by the contiguous, continuous, or remote, local or partial sympathies, as a continued derivative, after the manner of the acupuncture, or merely by keeping the outlets sufficiently open to give free vent to the discharges, we have abundant evidence of its utility in promoting the healing of caries, in accelerating absorption, in facilitating the separation of the dead from the living parts, and in arresting morbid osseous enlargements, to enable us to decide in its favour. Whether it exerts a specific power over the parts is equally matter of doubt. The disappearance of fungous growths, by continued applications to one particular part, would incline me to this opinion, which receives some confirmation from the character and termination of the first and sixth cases which I have described.

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* I have used the caustic after the same manner in the treatment of abscesses. In a case of empyema and a psoas abscess, where upwards of an English pint of matter was discharged daily for several days after the first partial evacuation, the caustic was regularly introduced to the depth of half-an inch into the openings at each dressing, and both patients got well. The constitution was supported at the same time.

Of the moxa I have not had much experience; but where I did try it, the results were not so satisfactory as those which have been reported by others.

The actual cautery I have used with advantage; and I can truly assert, that when it is applied with dexterity, and nearly at a white heat, the pain is by no means so great as might at first sight be imagined. Unless, however, the iron be brought to a sufficient heat, and adroitly applied, it will unquestionably aggravate the mischief, rather than prove beneficial.

When the excision of the part will not prove detrimental; when little prospect is entertained of effecting a cure by less formidable means; where the patient is passive in regard to the operation, and the part can be with facility removed, there can be no question with regard to the superiority of this measure over every other.

In all these affections, especially those of long standing, the effect of topical remedies will be powerfully aided by the use of those which invigorate the general health, and improve the state of the constitution. For this purpose I have found nothing to equal the sarsaparilla, exhibited in the form of its compound decoction. The formula employed was that of the Dublin college; and from the benefits witnessed under its use, I have no hesitation in asserting that the disappointments which others have experienced, and which have led to so many conflicting opinions regarding its medicinal virtues, may be justly referred to the badness of the article furnished by our druggists,—an assertion which may be verified by repeated trials of the several varieties usually offered for sale. That imported from Jamaica is decidedly entitled to a preference. In a conversation which I recently had with Dr Saunder and Mr Nautier, an eminent physician and surgeon in Rotterdam, a variety of this medicine was introduced to my notice, different from any which I had ever seen; and from their report must be much more powerful than that commonly used in this country. I could procure none of it either in London, Liverpool, or Glasgow; but having as yet made no trial of the quantity now in my possession I cannot speak as to its superiority.

In some cases where the decoction did not appear to do much good, tartarized antimony in solution, with tincture of opium, were given to such an extent as the stomach could bear without nausea or sickness, two hours after each potation, as was also the calomel pill every night, or second night at bed-time, with the best effects; nor was the same improvement observed by the use of the latter alone. In every case, therefore, where the decoction *per se* is found to be inert, it may be advisable to combine the others ere it be abandoned.

It is scarcely necessary to observe that the diet should be both light and nourishing, and that all salt and highly seasoned food should be studiously avoided.

As the object of the present essay is purely to communicate the results of the practice which I have pursued in the management of diseases, regarding which the sentiments of the profession have hitherto been much divided, I trust I shall be forgiven for omitting the mention of many valuable additions lately made to our literature on this important subject. A hope is entertained, however, that the treatment recommended, especially as it regards the use of the caustic and sarsaparilla, will be found abundantly satisfactory in the practice of others. Some cases may certainly get well by the use of either, but the curative process will more certainly be accelerated by the combination.

ART. II.—*Cases and Observations in Medical Jurisprudence. I. Imputation of Poisoning. II. Poisoning with Arsenic.* By ROBERT CHRISTISON, M. D. F. R. S. E. F. R. C. Phys. and Professor of Medical Jurisprudence and Police in the University of Edinburgh, &c. &c.

I HAVE often had occasion to regret that so little attention has been paid in this country to preserving, in sources accessible to medical men, the interesting medico-legal inquiries which are annually made throughout Britain. The want of authentic documents, embracing the medical facts and disquisitions which have been brought forward on trials in our own country, has obliged our medico-legal authors to illustrate the doctrines they have laid down, by referring to the proceedings in foreign courts of judicature. Valuable, however, as are the records which have been published of these proceedings, particularly by the medical jurists of Germany, they want, in the eye of the English reader, the interest possessed by domestic occurrences ; they are apt sometimes to convey incorrect impressions of the scope of medical jurisprudence and its influence on the administration of British law ; and I cannot help adding, that, in point of authenticity, the facts recorded must generally fall short of those determined in the course of judicial inquiries in this country ; for however imperfect our preliminary investigations may often be, the facts eventually brought out in evidence are in no other country so fairly stated, so minutely inquired into, and so completely put to the test as by the form of trial in Britain.

These considerations, and the growing interest taken by the profession in medico-legal science, have led me to conceive that an account of the most important particulars of such trials as may come under my own notice or that of my friends will not be unacceptable to the readers of a Journal that has always assigned to medical jurisprudence a prominent place among its objects. The plan I propose to pursue will be apparent from the examples which follow. I shall give an analysis of the whole proceedings in each trial, and dwell more fully on any particulars, which appear of interest, either by their influence on the case, or by the illustration they furnish of established doctrines in medical jurisprudence, or by the new views which they suggest; and I shall annex occasionally such observations and experiments as seem necessary to complete the practical instruction which it is my purpose to convey.

The first case is drawn up from documents for which I am indebted to the kindness of Mr Thackrah of Leeds, the principal medical witness; and the account has been subsequently revised by him. It is a masterly example of the detection of a False Accusation of Poisoning, for the full developement of which the prisoner was in a great measure indebted to the medico-legal inquiries of Mr Thackrah and Mr Walker. Few cases of the kind have occurred in Britain; yet this is not the first that has fallen under my notice. The investigations in the case of Whalley illustrate admirably the general plan of procedure in such circumstances.

The second case is drawn up from my own notes. It is interesting on account of the extreme minuteness of the quantity of arsenic detected, the quantity being less, I believe, than was ever detected before in medico-legal practice. It is likewise interesting, as it shows how much more skilfully medico-legal investigations are now conducted in country districts than they were but a few years ago; for the researches of the Arbroath gentlemen are, with some insignificant exceptions in point of form, complete in every particular. And it is farther valuable as being, so far as I know, the only example yet published, which will authorize a very frequent statement in general works on medical jurisprudence,—that arsenic causes perforation of the stomach.

Case I.—*IMPUTATION OF POISONING.—Detection of the conspiracy by the inconsistence of the evidence for the prosecution with the chemical analysis and with the effects of poison on the system.*

Samuel Whalley was indicted at York Spring Assizes in

1821 for maliciously administering poison to Martha King, a dress-maker, with whom he cohabited, and who was pregnant by him. The indictment contained three charges,—a charge of administering poison for the purpose of killing the woman, another of administering it for the purpose of destroying the living child in her womb, and a third of administering it for the purpose of procuring abortion, the child not being quick.

The moral evidence against the prisoner was derived almost entirely from the testimony of the prosecutrix King, and that of another woman, Merryweather, who lodged in the same house with her. Its purport was, that the prisoner had, under promise of marriage, cohabited with King, who became with child in consequence; that the marriage was put off, after proclamation of banns, on the ground of its being inconvenient at the time to the prisoner;—that on two occasions after this, and recently before she was poisoned, he tried to prevail on her to take drugs for the purpose of destroying the child, and maltreated her because she refused;—that she had a dream about purchasing poisoned tarts, and that a fortnight afterwards the prisoner brought her several tarts, which, while he declined partaking of them himself, he urged her to eat, "because he should like young Samuel (the *fœtus in utero*) to have one;"—that she did not take any that day, but ate some the following afternoon;—that after she had eaten them and was taken ill the prisoner paid her a visit, and in quitting her made a remark which implied he thought her dying, and knew she had taken something deleterious.

The prisoner on the other hand, while he admitted the cohabiting, the promise of marriage, and even that a day had been fixed for it, stated that it was broken off in consequence of his having caught her in a suspicious situation with another man;—that she frequently threatened his life on account of his refusing to make good his promise to marry her;—that he carried the tarts to her, because she asked for them on account of a curious dream she had about tarts;—and he proved by two witnesses that she had arsenic in her possession, although in her evidence she denied she ever had any or knew what it was. The defence was therefore grounded on a charge of conspiracy on the part of King and Merryweather.

The medical evidence was to the following effect.

According to the deposition of King, corroborated in many points by that of Merryweather, she felt an "unpleasant coppery taste" while eating the first tart, and this was so much increased while she was eating a second, that she did not take the whole of it. Half an hour afterwards she became sick, felt a burning heat and pain in the stomach, was seized with trembling and feebleness of the limbs, and subsequently had some vomiting. Her fears being in consequence excited, she sent for Mrs Merryweather, and along with her examined two tarts which remained; and they found a white powder hidden under the preserves. They became both alarmed, and Merryweather immediately set off in quest of a medical man. But it appeared, that notwithstanding her state of alarm, the messenger went to a gentleman who lived a mile off, who had never attended King before, and to whose house she did not know the way. Mr Thackrah, surgeon at Leeds, who was the gentleman called, arrived at Mrs King's house about six in the evening, two hours nearly after she had taken the tarts. He found her agitated, and complaining of the symptoms mentioned above; but "the pulse was not much accelerated, the tongue exhibited nothing particular; she did not complain of urgent thirst, or sense of heat in the mouth or throat; nor did she seem to have acute pain." As she told him she had not vomited, he immediately administered an emetic, and soon afterwards another, as the first did not operate. She then vomited. The matter discharged was preserved in a wash-hand basin, and taken home that evening by one of his pupils who had accompanied him; but Mr Thackrah admitted that the prosecutrix might have introduced a small quantity of poison into the basin before she vomited.

He left her about seven, and sent his assistant to see her at ten. Meanwhile she had, according to her own account, another attack of vomiting, the matter of which she preserved, and showed to Mr Thackrah at his visit next day; but she did not speak of it that evening to the assistant. This stuff was not taken away for some days, as Mr Thackrah did not happen to have any convenient vessel with him at his second visit. On this occasion he found her complaining of headach and pain in the bowels, and the tongue was furred; but she had no particular illness that was apparent, nor was there subsequently any of those effects which are usually caused by small doses of arsenic. After a few days attendance, deeming her well, he discontinued his visits. She, as well as Merryweather, however, stated at the trial, that she had been obliged to keep her bed several days, and that she had never been well afterwards.

The chemical analysis of the various suspected articles was conducted partly by Mr Thackrah, partly by Mr Walker, a chemist of Leeds.

In the two tarts, which were delivered to Mr Thackrah at his request on his first visit, they found under the preserves a white powder, which proved to be arsenic. Mr Thackrah remarked, that when a portion of the powder, with a little of the preserve adhering to it, was dissolved in distilled water and alkalized with subcarbonate of potass, the sulphate of copper caused a green and the nitrate of silver a yellow precipitate; and Mr Walker determined the quantity in each tart to be between four and five grains.

Mr Walker likewise examined the matters of vomiting and detected arsenic by the same tests in both. The quantity in the first was exceedingly minute,—not above one grain dissolved in at least three pints of fluid. The second matters of vomiting contained a much larger quantity,—namely between twelve and fifteen grains; and from the observations Mr Walker made, " he doubted very much whether the arsenic of *this* analysis had ever been in a human stomach." *

Such were the facts of this important case. The following are the conclusions which may be drawn from the medical evidence.

In the first instance, Mr Thackrah deposed on the trial, that, although the illness of the prosecutrix was undoubtedly caused by something which had disordered her stomach, he did not think her situation corresponded with that of a person who had taken so much arsenic, as was to be inferred from what was contained in the tarts analyzed;—and that, if he had heard nothing about poison, he would not have supposed she had taken any. He admitted at the same time that he could not affirm positively from the symptoms, that she had not taken a *minute* quantity of arsenic, as the symptoms of arsenical poisoning are subject to great variety.

In the second place, Mr Walker deposed, in regard to the coppery taste said to have been felt by the prosecutrix while eating the tarts, that arsenic could not cause such a taste, because its taste " *is sweet*, not like copper; if mixed with sweet tart, it could not be perceived at all."

Thirdly, According to the analysis of the powder in the tarts which were not eaten, the prosecutrix could not have taken *above ten grains* of arsenic in what she did eat. Now, even after repeated attacks of vomiting, the last matters discharged contained *at least twelve grains*; which clearly showed, that arsenic had been mixed with the stuff after it was vomited,—if, indeed, there was vomiting at all. The same inference is deducible with still greater certainty from the comparison of the quantity contained in the last matters of vomiting with that contained in what was discharged a short time before when vomiting was encouraged by emetics and the free use of diluents,—the latter quantity not being above *one* grain. The prosecutrix indeed stated that the matter from which the larger quantity was procured had been discharged into the same bason as that from which the former matters of vomiting had been taken,—and that a considerable white sediment had been left in the bason when Mr Thackrah transferred the contents into another vessel. But Mr Thackrah was sure that no perceptible sediment was left.

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* Mr Walker having died since the trial, I have not been able to procure all the particulars of his experiments. The fact that arsenic has not been in the stomach may be ascertained by finding, that the precipitates procured from the alleged matters of vomiting by the liquid tests, and more particularly by sulphuretted-hydrogen, do not yield empyreuma or exhale the odour of burning animal matter in the course of reduction.

Fourthly, Mr Thackrah deposed, that if so great a quantity had been taken, as to render it possible for twelve or fifteen grains to be vomited two or three hours afterwards, it would have occasioned the most violent symptoms, and probably death.

It is impossible to conceive more pointed evidence. The medical investigations were alone amply sufficient to decide the nature of the case.

The prisoner, as will be readily anticipated, was found *Not Guilty.* It may be satisfactory to add, that the prosecutrix King and the witness Merryweather were immediately committed on a charge of conspiracy. But the judge, considering the unfortunate state into which one of them had been brought by Whalley's means, and that they had signed a paper to the effect, that all the charges made against him were false and the result of a conspiracy against him,—acceded to Whalley's request to allow them to be discharged.

CASE II.—*POISONING WITH ARSENIC—Perforation of the Stomach—Detection of the Poison in very minute quantity—Inefficiency of the liquid tests.*

Margaret Wishart was tried at last Perth Spring Circuit for the murder of her sister Jean and her natural child, by administering to them arsenic or some other poisonous substance.

The deceased Jean Wishart was totally blind, and in consequence had been for some time a burden on her sister, the prisoner, who in particular always made ready her food. The blind woman had a lover in the person of a man Roy, a lodger in the house, by whom she had a child about four years before her death, and by whom she was again pregnant at the time the poison was supposed to have been given. This man was also on very intimate terms with the prisoner, and there was strong reason to believe that he cohabited with her. Such being the state of the household, it may be conceived, that the several members of it could not be on good terms with one another. Accordingly, although several neighbours bore testimony to the friendly disposition of the prisoner towards her sister, those who lived in the house with them deposed, that the deceased frequently complained of the usage she received both from her sister and from Roy; and she asked one witness to take her as a lodger, as on account of her pregnancy she was apprehensive of ill treatment at their hands, and even used the expression that " she would not get leave to live."

On the evening of Tuesday the 3d of October she was seen by a neighbour taking porridge to supper; the prisoner and a man who lodged in the house being present, but not joining her. About twenty minutes afterwards she felt unwell and blamed the porridge as the cause; and in a few minutes more she was attacked with vomiting. During her subsequent illness, which ended fatally on the evening of Saturday the 7th, she had not any medical attendant, except that on Friday, when she was taken in labour, a midwife was sent for to deliver her. Several people advised the prisoner to get a medical person to attend her, and the deceased herself asked her to do so; but she refused, saying that she could not afford it, and that all the doctors in Arbroath could not do any good. She likewise refused to send for another sister who lived in the same town.

A few weeks before her trial the prisoner added an important circumstance to the moral proof, which, it will be seen, was previously by no means strong, by attempting to suborn a witness. A young woman, who visited her in prison, said in presence of the jailor and others, that she had accompanied the deceased to various shops in Arbroath, to ask for poison, and that the deceased actually got arsenic at one of them; but on the trial she confessed that it was all a fabrication, and that she had been prevailed on to tell it by the prisoner's repeated importunities.

No evidence could be procured that the prisoner had either purchased arsenic or had it in her possession.

The medical evidence in this case rested chiefly on the morbid appearances and chemical analysis.

The account which could be procured of the symptoms was exceedingly imperfect, as it merely went to show that the deceased was affected with more or less vomiting, purging, thirst, and general uneasiness, from the evening on which she was taken ill till that on which she died; that she was delivered of a living child on Friday; and that on Saturday morning the limbs were cold and stiff. An important point regarding the symptoms, however, was ascertained,—namely, that they began within half an hour after a meal. The symptoms do not appear to have been violent. Such as they were, the witnesses for the crown all agreed, that they might be caused by arsenic, but admitted that they might equally be caused by natural disease.

The body was disinterred for examination on the 15th, eight days after death, by Drs Arrot, Sharpey, and Palmer; and the following is an account of the chief appearances described in their report. On opening the abdomen they found about a pint of red-coloured liquid in the cavity of the peritonæum, a small perforation on the anterior surface of the stomach, through which a small quantity of black fluid escaped,—the external surface of the intestines red and highly vascular,—the uterus not reduced to its natural size,—the other viscera healthy. The stomach contained a thick, opaque, brownish-black liquid, and a little solid vegetable matter. Its inner surface was very vascular, and marked in different places with dark brown spots of various sizes; and the villous membrane was abraded here and there. The intestines were internally very red and quite empty.

A portion of the stomach, including the perforation, together with part of the contents, was sent to me for examination and analysis. The stomach having been preserved in a bottle along with the contents, and the examination not having been made till the 21st, the vascularity had entirely disappeared, and the membranes had imbibed the fluid like a sponge; but the nature of the perforation was quite distinct. The aperture in the peritonæal coat, which was about the size of a pea, was surrounded by a dark, ragged margin, and the inner coats were more extensively destroyed around it.

The chemical evidence was decisive of the presence of arsenic in the fluid part of the contents, and still more in the coats of the stomach.

Drs Arrot, Sharpey, and Palmer made several solutions by diluting and filtering the contents, and by boiling portions of the stomach and filtering the decoctions. In none of these fluids did the ammoniacal sulphate of copper, or ammoniacal-nitrate of silver indicate the presence of arsenic. But the sulphureous test in all its forms gave satisfactory indications. After some preliminary trials with sulphuretted-hydrogen water, and the hydrosulphuret of ammonia with the subsequent addition of muriatic acid, they united the various fluids, and subjected them to the process which I have recommended.* The liquor was acidulated with acetic acid, and then subjected to a stream of sulphuretted-hydrogen; upon which a yellow precipitate was procured, that yielded by reduction with the black flux, in three different trials, patches of metallic incrustation. I regret that the limits of this abstract do not permit me to give more than the substance of the able report of the Arbroath gentlemen. The whole investigation reflected the greatest credit on them, but particularly the chemical part of it; for, if I may judge from my own experiments, there has never been a judicial inquiry in which so minute a quantity of arsenic has been so unequivocally discovered.

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* Edin. Med. and Surg. Journ. for July 1824, and Edin. Chir. Med. Trans. Vol. i.

In my own analysis I first formed a decoction of the contents and of the stomach separately, and subjected each, after acidulation with acetic acid, to sulphuretted-hydrogen gas; a lemon-yellow milkiness ensued, which on boiling gave place to a lemon-yellow flocculent precipitate, with brilliant brass-yellow scales intermingled. This, when collected from both fluids and dried and reduced in a small tube, gave an arsenical crust with all its properties fully developed; and the crust was converted by repeated sublimation into little octahedral crystals of oxide of arsenic, which I estimated as amounting to about a fortieth part of a grain.

As the decoction of the stomach still retained its yellow colour, I evaporated it, and procured a yellow precipitate in broad flakes. Knowing from experience that there was too much animal matter present in this precipitate to admit of the process of reduction being applied directly, I subjected it to a farther process, in order to destroy the animal matter. The impure sulphuret was acted on by nitric acid with the aid of heat, and consequently oxidated; the acids were then neutralized with potass, and the solution evaporated to dryness; the mass was then deflagrated in a glass tube, which process takes place gently, with effervescence and without burning, if the proportion of the nitre is not too small; the product was then dissolved out, exactly neutralized with nitric acid, filtered, a small portion tested with nitrate of silver, which gave a brick-red precipitate, and the remainder treated with a solution of nitrate of lead as long as any precipitation took place. The precipitate, which, besides other salts, contained arseniate of lead, gave a remarkably characteristic crust when mixed with freshly ignited charcoal and heated in a tube strongly with the blowpipe; and the crust subsequently gave octaëdral crystals when repeatedly sublimed.* The quantity of oxide procured by this process was not sensibly greater than by the former.

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* I have since been in the custom of employing a different process for getting rid of the animal matter *before* the precipitation of the sulphuret; and in consequence the sulphuret under all circumstances is procured in a state fit for immediate reduction. This improvement I shall soon have an opportunity of making public.

The prisoner was convicted of poisoning her sister. Many persons entertained doubts of the sufficiency of the moral evidence, and some attempts were made to procure her pardon on the ground of its inadequacy. She was executed, however, and died protesting her innocence.

ART. III.—*On Stricture of the Lachrymal Duct.* By THOMAS PRIDGIN TEALE, Esq. Surgeon to the Leeds Public Dispensary.

DURING my attendance at the Hôtel Dieu in 1823 I was highly gratified by the success of M. Dupuytren's treatment of stricture of the lachrymal duct.

In the operation adopted by that distinguished surgeon, the use of the canula of Foubert and Wathen has been revived; but their mode of operating has been considerably modified and improved, the operation of M. Dupuytren consisting simply of inserting into the lachrymal canal a metallic tube, over which the integuments are to be healed. The general success attending the operation was such as particularly to arrest my attention; but I am unable from my own observation to state the average number of cases in which it proved successful. M. Dupuytren, however, states the result to be favourable in nineteen cases out of twenty.

Although by reasoning upon the subject we may be unable to conceive "how a metallic tube can be expected to form a substitute for the natural duct, an inorganic in lieu of an organized part, in perpetuity," * yet, when experience is opposed to speculation, the latter should certainly be abandoned.

Various measures have been at different times adopted for relieving this disease, but all of them have proved more or less unsatisfactory.

The antiphlogistic treatment in favourable instances frequently relieves the thickened state of the membrane lining the duct, and the passage for the lachrymal secretion sometimes becomes restored. But this plan of treatment often fails to relieve the obstruction, even when pursued for a considerable length of time; and in those cases where it proves successful, the progress of the cure is often tedious, and requires considerable patience and perseverance both on the part of the surgeon and his patient. The various methods devised for dilating the passage by probes and other mechanical contrivances, and for re-establishing the canal by setons, are objectionable from the frequent failures which occur, and from the repeated operations which many of them require. The employment of the nail-headed style, which probably is the operation most frequently adopted by British practitioners, is likewise attended with considerable trouble and inconvenience. In the early days of its employment, the removal and re-insertion of the style, which from time to time is necessary, cause considerable annoyance, if not pain to the patient; and even when the final result is favourable, permanent deformity is produced, either from the establishment of a true lachrymal fistula, or from the instrument being visible at the inner angle of the eye.

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* Travers's Synopsis.

Over these different methods of treatment the operation of M. Dupuytren, although not entirely free from objection, nor invariably successful, possesses, in my opinion, numerous advantages.

1*st*, In the facility of its performance.

2*dly*, In the immediate and complete relief of the disease, at least in an immense majority of cases, M. Dupuytren stating (as I have before observed) the result to be successful in nineteen cases out of twenty.

3*dly*, In the prevention of any permanent deformity, it being nearly impossible in most instances to discover on which side the operation has been performed.

The operation which I am about to describe is undoubtedly well known to numerous British surgeons, and is probably practised by several. But as it is far from being generally adopted, or even known to the greater part of the profession, I have been induced to drawn up the following description of it, and to lay before the public the result of the few cases in which I have adopted it.

The instruments employed in the operation are a common French bistoury, a canula of silver, gold, or platina, and a *porte-canule,* to which the canula is affixed for being more conveniently inserted in the lachrymal passage. The length of the canula is one inch; its upper opening measures one-tenth of an inch; the tube gradually tapers from its upper to its lower opening, the diameter of which measures one-twentieth part of an inch; the lower opening is placed obliquely, and the upper opening is surrounded by a small rim to prevent its sinking too low into the duct.

The following is the manner in which the operation is performed. The patient being seated in a chair, rests the head against the breast of an assistant, who fixes the head by a hand placed over the ear and temple on each side. The operator (supposing the left eye to be operated upon) stands in front of the patient, and a little to the left side. With the index finger of the left hand he puts the integuments at the inner angle of the eye upon the stretch, for the purpose of rendering the tendon of the *orbicularis palpebrarum* more evident, at the same time that the assistant, who holds the head, stretches the integuments at the outer angle of the eye. The lachrymal sac is then to be punctured by the point of the bistoury being inserted immediately below the tendon of the *orbicularis palpebrarum.* The direction in which the instrument should be inserted is nearly vertical, the handle inclining slightly to the opposite side, and a little forwards, on account of the projection of the eyebrow, the edge of the blade being directed obliquely downwards, or towards a point situated midway between the angle of the mouth and the pinna of the ear, this being the direction which admits of the bistoury being insinuated to the greatest extent down the lachrymal canal. The puncture being made, the bistoury is allowed to remain as a director, and the operator taking in the left hand the porte-canule armed with the canula, gently insinuates the tube along the side of the bistoury into the lachrymal canal; the bistoury is then withdrawn ; slight pressure is made upon the upper part of the canula, so as to cause it to be completely imbedded in the canal. The operation now being completed, the patient is directed to blow down the nose at the time that pressure is made upon the nostrils, by which the tube is freed from any blood that may have accumulated in it, and the air issues out at the incision, showing the free communication that is established between the nostril and the sac.

Case I.—February 1827.—Martha Soward, æt. 25, has been long troubled with severe attacks of inflammation of the lachrymal sac, and in the intervals has suffered much inconvenience from the epiphora which remained. At present the sac is distended ; the integuments inflamed, and conjunctiva red. Slight relief has occasionally been procured by the application of leeches, wet cloths, and fomentations, and by the use of aperient medicines. For the last few weeks the same treatment has been adopted, but with less relief than usual.

February 11.—Canula inserted.

12.—Considerable swelling and pain in the lower eyelid and cheek. Ordered to apply leeches and fomentations, and to take a dose of senna and Epsom salts.

16.—Inflammation diminishing.

21.—Swelling subsided ; conjunctiva less inflamed than it has been for weeks previous to the operation ; the tears continue to flow over the cheek.

27.—She still experiences slight attacks of inflammation, and the tears flow over the cheek. From these circumstances I supposed the tube must have been obstructed. The tube was consequently removed and a fresh one inserted.

May 20.—She feels no relief from the second operation; the ophthalmia occurs with its former violence and frequency, and the epiphora remains undiminished; the head of the tube is protruded upwards against the cicatrix. An incision was now made into the sac, and the tube extracted without any difficulty. The patient has since left the town.

I believe the employment of Ware's style would in this case have been equally unsuccessful from the great irritability of the parts.

Case II. February 1827.—Mary Hudson, æt. 30, four years ago suffered from inflammation of the left lachrymal sac; suppuration took place; the matter was evacuated by puncture, and the wound closed in about nine months. Since the commencement of the attack the tears have continued to flow over the cheek, and the sac has repeatedly become distended. The principal inconvenience of which she complained was the dimness of vision which frequently occurred from the excess or accumulation of the lachrymal secretion on the globe of the eye and between the palpebræ.

Feb. 20.—Tube inserted.

21.—Wound united; slight swelling of the lower eyelid.

24.—Swelling subsided; wound healed; tears flow down into the nostril; she feels perfectly relieved of the annoyance she has so long experienced.

October 11, 1827.—She has continued perfectly free from inflammation and epiphora since the operation.

Case III.—May 1, 1827.—James Smith, painter, æt. 39, has been troubled with watery eye during seven or eight years. During the last two years there has frequently been a tumour at the inner angle of the left eye, which for three months has been permanent, and sometimes as large as a small nut. The watering of the eye has caused him much inconvenience, and when employed in his business he is repeatedly under the necessity of wiping the tears from the eye and cheek to prevent the dimness of vision which the presence of the tears occasion.

May 1.—Canula inserted.

2.—Considerable swelling of lower palpebra and cheek; slight pain. Ordered to apply rags wet with cold water, and to take an aperient.

6.—Swelling entirely subsided; dimness and watering perfectly relieved.

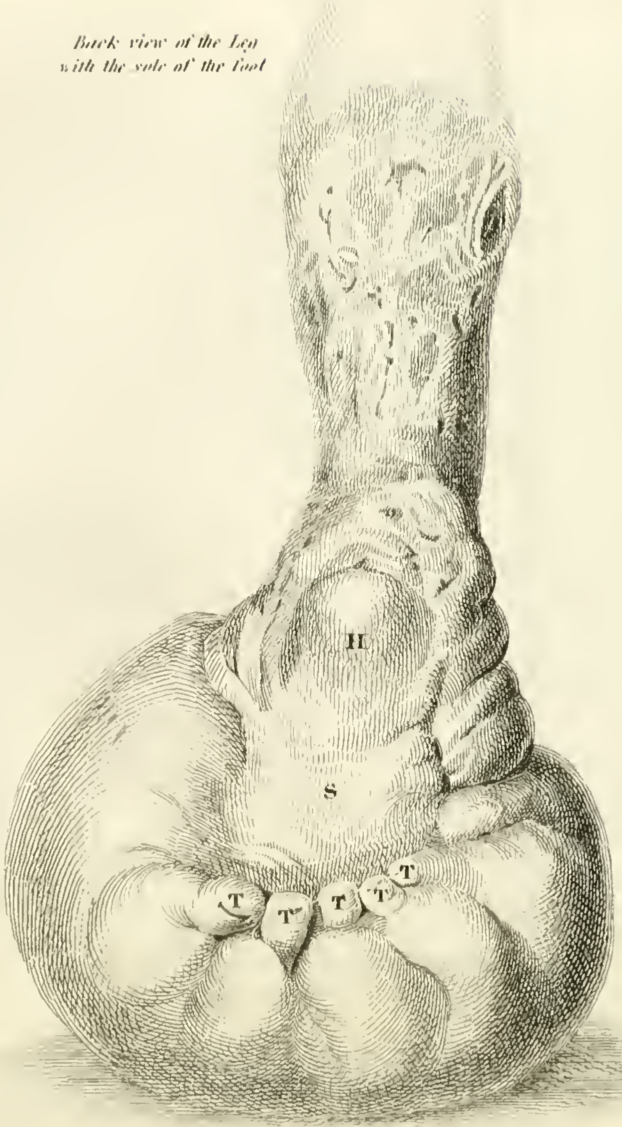
October 11.—Has not experienced the slightest inconvenience since the insertion of the tube. On showing him to-day to some medical friends, they were unable to say on which side the operation had been performed.

Case IV.—September 11, 1827.—Hannah Proctor, æt. 27, has had obstruction of the left nasal duct during six months, and has suffered great inconvenience from distension of the sac and dimness of vision.

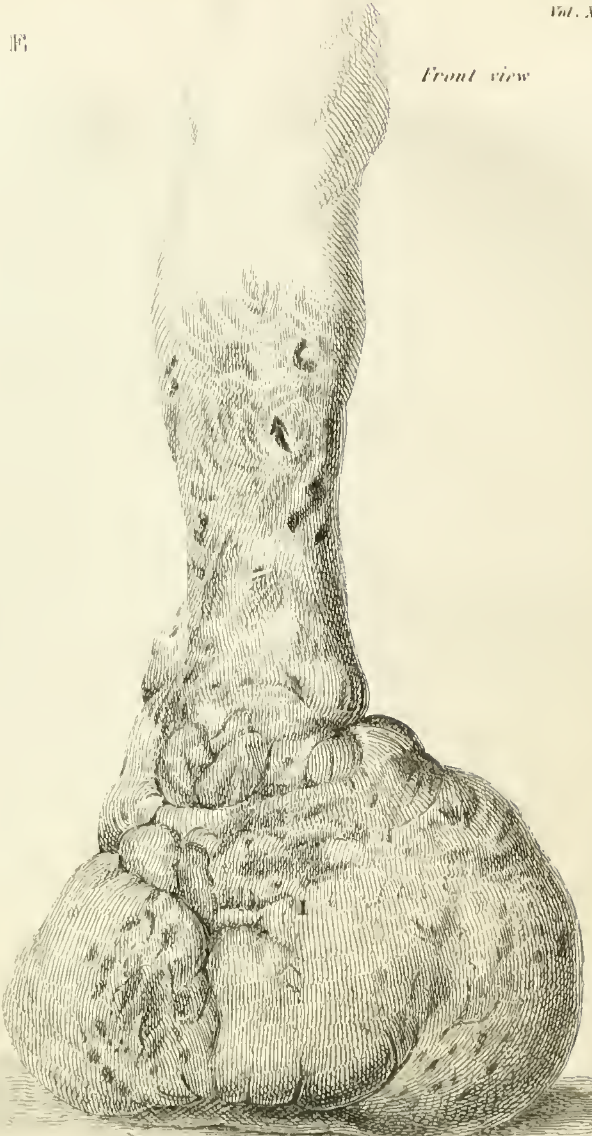
Edin^r. Med. Journ.
Vol. XXIX p. 31.

M^r. HULL'S CASE
OF ELEPHANTIASIS
Arabum.

Back view of the Leg
with the sole of the foot



Front view



References.
S. *Sole of the foot.*
T.T.T.T.T. *Toes.*
(*lower surface.*)
H. *Heel.*
I. *Instep.*

Published by A. Black Edin^r. 1827.

W.H. Lizars sc.

September 11.—Tube inserted.

13.—Wound healed; no swelling; epiphora relieved.

18.—Continues perfectly well.

Case V.—September 19, 1827.—Mary Dixon, æt. 72, had inflammation of the left lachrymal sac nine months ago, and since that time the tears have constantly flowed over the cheek. She has suffered considerable inconvenience from frequent attacks of ophthalmia, distension of the sac, and dimness of vision. The sac has never suppurated.

September 19.—Canula inserted.

21.—Slight redness and swelling of lower eyelid; dimness of vision and epiphora perfectly relieved.

26.—Inflammation quite subsided. She expresses her gratitude in the warmest terms for the relief she has obtained.

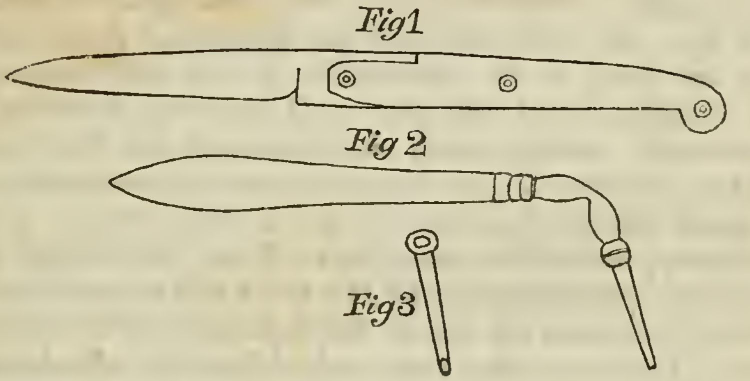


Fig. 1. The bistoury.

Fig. 2. The porte-canule, consisting of a style adapted to the size of the canula, and a handle placed nearly at right angles with the style.

Fig. 3. The canula.

22, *Albion Street, Leeds,*
October 20, 1827.

Art. IV.—*A Case of Elephantiasis Arabum, with remarks.* By Robert Hull, Member of the Royal College of Surgeons. *(With a Plate.)*

In the commencement of September 1820, I saw Maria Carr, aged 32, who laboured under a great enlargement of the right foot. The morbid growth, as may be seen in the accompanying engraving, occupied its upper surface, and projected a great deal beyond the toes and laterally. By its weight it had completely extended the foot; the sole was turned quite backward; and the portion of tumour beyond the toes came into contact with the ground when she dropped the limb. A string passed round the tumour and sole measured two feet and a quarter of an inch; round the part projecting beyond the toes two feet five inches and a half; from the bend of the instep to the extremity of the tumour ten inches. The integuments of the swelling and of the leg, nearly as high as the knee, were discoloured, in some parts of an erysipelatous redness, in others ulcerated and scaly; they presented indurations and hollows which, according to the tale of the woman, indicated the sites of former abscesses. The motion of the ancle-joint was destroyed, and habitual flexion of the leg had left but very little to the knee.

The patient stated that thirteen years ago she struck her shin; that ulceration followed; and that ever since some portion or other of the limb had been ulcerated. That seven years ago she *suddenly felt an agonizing pain extending from her knee to the extremities of the toes, accompanied with vomiting and diarrhœa:* That this diarrhœa lasted three weeks, during which she perceived her foot was swollen, but that she forgets how soon after the commencement of the attack the swelling was remarked. She was, if I remember rightly, bed-ridden many days. That the swelling went on to increase, permitting her for two years still to walk, but totally disabling her from the use of the diseased limb for the last five years: That after the tumour had become considerable she suffered at times exquisite pain therein: That occasional hæmorrhages from the surface of the swelling were of terrific character. As her health appeared to suffer seriously, I proposed amputation, which was performed in the presence of my friends the late Dr Alderson, Messrs Scott, Johnson, and others, above the knee; for although the leg was of its natural size, the morbid state of the integuments rendered its amputation ineligible.

It was performed on the 29th of September; and of the treatment of the stump I have to remark, that I was not permitted to apply a roller, the woman declaring she could not bear the pressure. A projection of the thigh bone was, as I expected, the ultimate consequence. Yet, on the whole, the state of the patient was favourable. She had even walked about her room on crutches, when, on the 16th of October, she had a rigor. On the 19th, when I first saw her after the rigor, I remarked much constitutional irritation, with nausea and vomiting, a pulse of 158 strokes in the minute, *the stump covered with an erysipelatous redness*, and the wound, maintained by the projecting thigh bone, discharging a lymph-like fluid. On the 20th, the erysipelatous redness was fading, but the stump was swelled to about one-half as much again as its proper size; it was tender when touched; the granulations had become white, resembling distended vesicles, and the lymph-like fluid was still discharged. 21st. I discovered that the disorder had attacked the *pudenda externa;* and she now confessed that these parts first swelled after a rigor on the 19th. The labia were monstrously enlarged; a tumour, which seemed to occupy the site of the *glans clitoridis*, equalled the ends of two fingers; there was enlargement within the labia; and many tumours, some pediculated, occupied the perinæum and neighbourhood of the anus. The *mons veneris* was very large, and covered with erysipelatous redness, which extended to the groin. The swelling of the *mons veneris*, she said, had existed about four years; and the hair on that region was very scanty and devoid of colour. 22d, I snipped off two of the smaller tumours, when lymph-like fluid exuded from the cut surfaces. She complained of relaxation of the uvula, which excited efforts to vomit. The reader will perceive how curious was this uvular affection, as existing contemporary with that of the genitals. The integuments of the *mons veneris* and groin now assumed the peculiar dusky red colour of the amputated limb. The cuticle on the hands I observed to be in a scurfy state. She had some liquid stools this day, and on the 24th the labial swellings were lessened. 25th, Discharge from the stump duly purulent. 27th, She prognosticated a rigor from "the vapours" which used to precede, and I prescribed a drachm of laudanum. No rigor occurred. The great constitutional irritation was diminished on the 29th; her pulse beating 96. About this time I thought that I prevented a rigor, by a scruple of the *pulvis jalapæ compositus*, which excited vomiting.

I am now arrived at an interesting stage of this disorder. On the 2d of November the patient voided from the bowels a lymph-like fluid, similar to that which had flowed from the stump, not small in quantity, and containing lumps perfectly white and jelly-like. The excretion was preceded by pain in the bowels. I had said she had not menstruated thirteen years; but she has had at times "a rush" of white or yellow discharge from the vagina, which, by her own account, resembled what now was evacuated from the anus. It resembled also what on dissection of the amputated limb I had found in the cells of the cellular membrane.

Nov. 4th.—She had much hypogastric pain, which was relieved by purgatives; and on the 5th she voided more of the lymph-like fluid.

Nov. 7th.—She passed a small stool, bloody, and preceded by tormina.

8th.—The abdomen was soft, not tender but on extreme pressure; yet all ingesta excited immediate abdominal pain and vomiting. The confection of opium gave some relief; but the lymph-like evacuations still followed each paroxysm of pain.

11th.—Pain, nausea, and vomiting less; the countenance less anxious; pulse 106. On the 16th, an excessive pain attacked the loins, the pulse beating 150; and an erysipelatous eruption like those already noticed appeared on the back and loins. The constitutional irritation and velocity of pulse continued the following days, but on the 20th the erysipelas had faded from the back, and the pulse fallen to 100.

December 1.—She was attacked with dyspnœa, for which a blistering plaster and anodynes were prescribed. On the 2d, the dyspnœa had disappeared, but the pulse was nearly gone, the face Hippocratic, and at four in the morning of the 3d she expired.

Fifty-six hours after death I examined the body.

The right lung was adherent to the costal pleura by long bands; its substance not a little solidified; and about a pint of serum was effused into this cavity of the thorax. The texture of the heart and great vessels was *very* lacerable.

The abdominal viscera were pale externally. Lymph had been effused from the peritoneal coat of the spleen, producing partial adhesion. There was no ulceration in the intestines; no mesenteric disease. There was an abscess of the left ovary; puriform effusion into the uterine cavity; none into the abdomen or pelvis.

I have intentionally reserved to this place an account of the dissection of the leg, which I performed at the time of amputation. The integuments were much thickened; and the bulk of the tumour formed in the subjacent cellular membrane in some places nearly as dense as cartilage, and similar in appearance. The extremity of the *tendo Achillis*, the cellular substance, the annular and lateral ligaments of the ancle, were blended in one confused mass. About a pint and a half of lymph flowed from the cells between the solid tumour and the muscles and tendons of the foot. The ulcers on the leg had affected its muscles, which were moreover pale and attenuated. The bones of the leg were nodose, and the periosteum was easily detachable from their roughened surface. There was deficiency of animal matter in these bones, but the bones of the foot were especially devoid of it. Anchylosis was nearly complete in the ancle-joint.

This case confirms the opinion advanced by Hillary of the impropriety of amputation in this disorder, which is evidently what he termed elephantiasis, and what Hendy called the glandular disease of Barbadoes. From this history it is evident that the organs concerned in the production of the swelling were the lymphatic vessels alone; for the woman averred she never had enlargement of the inguinal glands, regarded by Hendy as the more immediate agent in this disorder.

This case likewise corroborates the opinion of Hillary, viz. that the accompanying fever in Elephantiasis Arabum is not symptomatic, as Hendy supposed, but rather essential, the swellings being the result of a critical deposition.

The woman Carr affirmed that accessions to the size of the foot were always sudden, and preceded invariably by the febrile symptoms. The train of symptoms which appeared after amputation in her case remind me likewise of a remark by Hillary, who says, " I once saw a patient where this morbid matter was cast upon the scalp, the ears, and the back part of the neck; and another, wherein the matter was cast upon the lower part of the *spina dorsi*, the *os coccygis*, and the lower part of the loins, at each time of the return of the fever, which was attended with all the same symptoms as when it falls upon the leg."

The character of metastasis, or rather of misplacement assumed by the disorder of Carr in consequence of amputation, was witnessed by Hillary to result from another cause. This physician, in deprecating blood-letting, says, " it hinders nature from critically discharging the morbid humour upon the leg, and sometimes turns it upon the vital parts, and proves fatal, as I have more than once seen it."

Even Hendy had made observations, which, had not his mind been pre-occupied by theory, would have led him to a truer conception of the disorder. He says, " I have been thrice called to patients, in whom the bowels have been affected. One of these, being at some little distance from town, died before I could possibly reach the place; and another lost his life before my prescription could be made up. The third lived but a short time after I had seen him. They were all free from any complaint, except the glandular disease, and were all in health a very few hours before I was sent for."

I cannot doubt that by amputation I accelerated the death of this patient. But I was at that time ignorant of the true character of Elephantiasis Arabum. I had not seen Hendy's Treatise, nor was I aware of the experience against surgical interference which Hillary had asserted, who says amputation " has been often performed, but always without removing the disease; for the fever has certainly returned, and the morbid matter has as constantly fallen upon the other leg, and produced the same effects."

On the nosological character of this malady, the name *elephantiasis* has, I believe, led medical men into an error, which is important in proportion to the frequency of the disorder. They confound the Elephantiasis Græcorum with the Elephantiasis of the Arabians; but these had applied the term to what the Greeks appear never to have noticed. The Grecian elephantiasis was the leprosy of the Arabians: the Arabian elephantiasis was that disorder, of which the "Barbadoes leg," the "Cochin leg," of modern writers are forms. The peculiarity of the latter malady is, that the monstrous elephantine swellings are not the concomitants of a cachectic state, slowly developed as in the Arabian leprosy; nor are they engendered like sarcomatous tumours in healthy frames. But they are formed with repeated attacks of smart fever. Upon every attack there is fresh deposit of tumour; and there is every degree of the disorder, from a slight swelling, which may subside entirely, up to the huge masses, which have certainly, in this disease, derived the name from their bulk and ugliness.

This disease is not so rare as most persons imagine. It may be more general in the West and East Indies; but it is frequent even in Ireland, as may be seen in a paper by Dr Graves in the recent volume of Dublin Hospital Reports. Dr Graves does not think his cases are those of true Arabian Elephantiasis. I believe they are specimens of the same affection, of which the symptoms are accurately and convincingly defined in a work very rare in this country, but published in Paris in 1806. I allude to the production of the celebrated M. Alard,—a work which displays much ingenuity, extensive reading, and philosophical research; and which contains the most accurate history of the phenomena, and the most luminous view probably of the pathology of this disease yet extant. *

I had proceeded thus far with my remarks, when I met with the excellent paper of Dr A. Musgrave "on the Unmixed effects of Mercury on the System." It is contained in a recent number of this Journal, and is, I believe, the very first publication which announces confidently remedial measures for the remarkable and important malady of which I have been treating. Dr Musgave would term it the "migratory inflammation of the lymphatic system." But it has been denominated elephantiasis by so many modern authors, that it is highly desirable to retain the name, provided this can be effected without confounding the disorder with the elephantiasis of the

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* Histoire d'une Maladie particulière au Systeme Lymphatique, frequente, quoique meconnue jusqu'a ce jour, par M. Alard."—This was published in 1806, and in 1811 a further account was printed, entitled "Nouvelles Observations recueillies sur l'Elephantiasis des Arabes."

Greek writers on medicine. This may be done by adding the special term *Arabum* to the common designation elephantiasis. There is abundant evidence that the Arabian physicians were acquainted with this lymphatic malady; that they called it elephantiasis; and that the Greeks gave this name to the lepra of the former. Conceiving this disorder to merit more attention than European practitioners have hitherto bestowed upon it—that it is more frequent than is generally believed—that the treatment hitherto published has been nugatory or pernicious—I earnestly recommend Alard's book for communicating a just and comprehensive notion of the disorder, and Dr Musgrave's paper for its successful treatment. Neither Towne, nor Hillary, nor Hendy, its first describers in the Western Indies, nor Alard, nor others in Europe, present any certain preventive or remedy for the fever. None of them advise, as does Dr Musgrave, the *mercurial influence*, of which he says, "I remember no case of fatal metastasis after swelling of the gums had been unequivocally observed." For the local and permanent deposition of lymph, when of magnitude, I should entertain little hope of a cure; yet in a case recorded many years ago in the London Medical and Physical Journal, Mr Ward of Manchester effected absorption of a vast swelling by the employment of pressure by means of adhesive straps.

Norwich, October 29, 1827.

ART. V.—*Physiological Experiments and Observations on the cessation of the contractility of the Heart and Muscles in the Asphyxia of Warm-blooded animals.*[*] By JAMES PHILLIPS KAY, M. D. House-Physician to the Royal Infirmary Edinburgh, and lately Senior President of the Royal Medical Society. No. 1.

RESPIRATION can be suspended for a very short space of time ere alarming symptoms supervene. Vertigo, with a sense of cerebral oppression, are premonitory. To these succeeds an internal agony of the chest, gradually aggravated, until, in cases in which the experiment is voluntarily made, it is arrested by the inward conflict which subdues the most resolute. The surface at a somewhat later period becomes livid; the cerebral faculties, and motion and sensation are abolished; and the organic heat gradually fails first in the extremities, afterwards in organs where the circulation is more vigorous, and there is less exposure to the atmospheric temperature. To a superficial observer all the functions seem to have expired from the moment in which the individual lost the faculties of sensation, motion, and his intellectual consciousness; but the vulgar are deceived by appearances in regarding this state to be actually death. The functions of the various organs may often, after this state has been for some time established, be restored to their natural and healthy standard. The contractility of the muscles and the action of the heart continue vigorous during a considerable time; and in other structures the apparent death is not real. Though the action of many organs has been changed into one which is incapable of giving evidence of life by its usual phenomena, an action continues which is capable of supporting a life peculiar in its phenomena, until, by the use of means to restore the function of the lung and circulating system, the natural actions are resumed. In a similar way a depression of the skull may suspend voluntary motion and sensation. When the depression is raised they are restored. The motive power, however, still continued in the muscles, but was not exercised from the want of a volition; and consequently no evidence of the surviving motility was evinced, unless the nervous influence had been excited by galvanism.

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[*] Read lately before the Royal Medical Society, and communicated by Dr Alison, Conjunct Professor of Institutes of Medicine, to the Medico-Chirurgical Society of Edinburgh.

In the same way we may conceive that motions might be excited in the nerves of sensation by external stimuli, which would have so affected the cerebral organs as to have produced sensation, had not the action induced by these objects in the brain been so morbidly changed, or so feeble, as not to have affected the mind, so as to cause the phenomenon which we term sensation.

We can conceive no change to take place in any organ by which its function is supported without a motion; but whether this happen in simple fibres, or in secreting vessels, or in organic molecules, it is not necessary at present to inquire. That motion which supports the most healthy condition of the organ must be the best, and therefore there can be but ONE such action.

Every other must be less capable of supporting the function; and in proportion as they differ from this they will tend to suspend it. They become morbid; and we may conceive them to become so much so as altogether to change the result of the organization, and thus to pervert the function of the part. This we may conceive concerning every tissue in the body. The phenomena of all may be thus changed; and though it is improbable that organs, the accordance of whose action supported a life of certain phenomena, would be capable of assuming different actions, and yet preserve again the balance of their activity, so as to act together for any great length of time in the support of a life of different phenomena, yet we may conceive this to happen for a considerable period.

In this inquiry, therefore, we shall examine the minute detail of the phenomena, in order to acquaint ourselves with the manner in which each individual organ is affected. In this morbid state we shall find actions to continue for a very considerable period in all; in some, beyond the period in which any instance of the resuscitation of natural phenomena has occurred. Asphyxia appears to be a mode of life which has few of its usual phenomena, but in which actions proceed during a certain period in all the organs, which, by peculiar methods of treatment, are susceptible of being changed into those which exhibit the usual vital phenomena.

Death appears to us to consist in the disunion of the intellectual principle from the corporeal organization.

This connection is maintained by various peculiar intestine motions in the various organs, of which one is the most perfect, and others are in various degrees morbid. General arguments appear to indicate the brain to be the intimate residence of this mysterious tenant. The intestine actions of the cerebral organization may be primarily or secondarily affected, that is, by the previous lesion of other organs, or without that lesion; but until these organic motions become such as to disunite the immaterial intelligence, life continues, though none of the usual evidence be given of its existence by organic motions. But as we cannot conceive, that when once the union between mind and matter has ceased, the intellectual principle can be recalled to the corporeal organization, all states in which resuscitation is said to take place must be only apparent, and consist only in a change from those morbid actions which suspended the usual phenomena of life, to those natural organic motions by which its usual functions and phenomena are supported.

Asphyxia differs from death, therefore, as in it the minute actions of tissues continue such as to support the connection with the intellectual principle. Although in death, as after decapitation, (when we cannot conceive the intellectual principle to be connected with the corporeal organization,) contractility and nervimotion long survive, and minute organic movements are vigorous, yet they have lost that portion of the organization which maintains the union with the intellectual principle. Whilst life yet remains the minute actions may be so morbid that no means can revive those by which life can be supported for any period, and then death is certain. But the separation of the intellectual principle from the corporeal organization may take place long before all motion has ceased, and then death has ensued; yet, as life may be restored in the early stages of asphyxia, it follows that death has not happened, and that this state must be regarded as life peculiar in its phenomena.

The first effect of interrupted respiration is to impede the progress of blood through the minute vessels of the pulmonary tissue. In the rapidity of its circulation there is a marked difference even in inspiration and expiration. Much might here be said concerning the pulmonary circulation in the different mechanical states of the lung, proving that it is retarded when the lung is comparatively collapsed, and *vice versa;* but the disquisition seemed to encumber this inquiry, and it has therefore, by the suggestions of a learned critic, been omitted.

When the access of atmospheric air to the lung is in any way prevented a short interval elapses; the animal makes an attempt at inspiration, resembling a deep sigh, which is necessarily ineffectual. This is followed by rapid gasping, by which the whole body is agitated, and every muscle which has power in respiration is most violently convulsed to overcome the obstacle to the admission of air. The surface and mucous membranes become livid, the sensorial faculties are abolished, and at the same moment the action of the voluntary muscles ceases. The circulation and the action of the heart continue for a short period only. On more minute examination the whole capillary system is found to contain only dark fluid blood; the left auricle a considerable quantity; the left ventricle scarcely more than is sufficient to moisten its parietes: the arteries are empty, but the venous system, especially where it empties itself into the right heart, is gorged. The right ventricle, and auricle, and pulmonary artery, are distended to the utmost with a dark fluid blood. If the asphyxia have been produced gradually the colour of the blood is darker, and the engorgement of the lung more perfect; and this always differs in proportion to the length of time in which it has occurred; but the left ventricle and arteries are invariably empty. On this subject the opinions of Haller, Kite,* (p. 56.) Coleman,† Bichat,‡ Orfila,§ agree in the general fact, differing somewhat in particular points.

This state of the circulation proves that the principal obstacle to the progress of the blood must be sought in the lung. Bichat, whose opinions on asphyxia led him to attribute much to the influence of the dark blood upon the heart, assents fully in the truth of this conclusion. ||

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* Elementa Phys. Vol. iii. p. 249.

† Suspended Respiration, p. 6 and 7.

‡ Sur la Vie et la Morb. p. 220.

§ Dict. de Med. Asphyxie, T. iii. p. 60.

|| Sur la Vie, &c. p. 227.

Besides the mechanical effects of air upon the lung, it has well known chemical relations with the blood, which must influence powerfully its progress in the pulmonary tissue. The minute vessels of all structures have peculiar sensibilities of organization, probably from the difference of their innervation, by which, in a healthy state, they circulate fluids of certain qualities only, and resist the introduction of any fluid foreign to their tissue by mechanical violence, so long as its peculiar sensibilities continue active. Thus, contiguous tissues circulate in their minute vessels fluids of very different qualities without commixture, and some in different degrees of the same succession of vessels always contain different fluids. In the lungs minute vessels exist, which convey the venous blood to the ultimate membrane of the pulmonary cells, where it is exposed to the atmospheric air. Changes ensue in its transition through this space which alter its qualities. The dark venous character is lost; it assumes the arterial red. It has more oxygen, and less carbon proportionally; and therefore, *a priori*, we should be induced to conclude, that the innervation or organic sensibilities of the minute vessels which circulate the arterial blood must be different from those which convey the venous. Reasoning from the general laws of the economy, it must have been regarded singular, had the vessels of arterial blood in the pulmonary tissue been capable of conveying blood, with equal velocity, in all stages of the venous degeneration. In suspended respiration we should expect, that, so long as the oxygen contained in the pulmonary cells should remain unexhausted, the ordinary changes of the blood would ensue, and the circulation for a period would continue with its usual vigour; and in proportion afterwards, as the oxygen diminished, the blood not undergoing that change which would be necessary, in order that it might permeate the vessels of arterial blood in the pulmonary tissue, that its circulation would be impeded. As the change, however, is only gradual, and the susceptibilities of action in all tissues are great, the venous blood would doubtless penetrate with diminished velocity the pulmonary organization, until its characters should be so greatly altered, as no longer to be capable of exciting the action of the vessels of arterial blood. If, then, air were admitted it would re-oxygenate the blood exposed to its action in the pulmonary cells, and bestow on it those qualities which are necessary to its circulation.

An experiment of Bichat is beautifully consonant with these conceptions.* "If a stopcock be adapted to the trachea, exposed and divided, and afterwards it be shut, the blood becomes darker, and springs for some time with its usual force" from a divided artery, " but at length the jet becomes gradually feebler. Readmit the air, the blood becomes almost immediately red, its saltus also rapidly increases." The blood, it will be remarked in this experiment, was observed to flow from an artery dark, and must therefore have permeated the lung without undergoing the usual changes. This, however, continues for a very short period.

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* Sur la Vie, &c. p. 222.

Exp. 1.—The trachea of a large rabbit was tied, the abdomen and chest opened, and at the end of the second minute from the commencement of the experiment the external iliac artery was divided, a considerable quantity of dark blood flowed, but at the third minute it had almost ceased to escape. The heart continued contracting vigorously; very small quantities of dark blood collected slowly every twenty seconds at the extremity of the artery. In five minutes all flow of blood had entirely ceased. The left heart contracted spontaneously for a very considerable period longer.

I repeated this experiment with similar results. In experiments of this nature I felt aware that the cessation of the arterial action might be attributed to the quantity of blood, which escaped when the artery is divided, so very soon after asphyxia has been produced.

Exp. 2.—I therefore tied the trachea of a small hare. The chest was opened. During three minutes the blood issued *per saltum* with a gradually diminished force from the mammary arteries, and then ceased. On putting my finger on the aorta I could discover no pulse. The aorta was divided, and a very small quantity of blood flowed, and immediately ceased. After three minutes and a half had expired no blood flowed from the aorta; little blood had been lost. The heart continued vigorous the usual period, and all the other circumstances of asphyxia were observed as usual.

This experiment was confirmed by subsequent observations. I however varied it thus.

Exp. 3.—A rabbit was asphyxiated by tying the trachea. The chest was opened. At the end of three minutes and a half no pulse could be discovered in the aorta. The left venous sinus was then opened, the blood contained escaped, and blood from a period of from one to two or three minutes occasionally collected in very minute quantities, as though it gradually drained from the larger vessels of the lungs, but never, as often as the experiment was repeated, collected in quantity. The heart continued vigorous the usual period, and all other circumstances of asphyxia were observed.

The supply of blood from the lungs to the heart is arrested, therefore, a very short period after asphyxia has been produced. But in the experiment of Bichat, when the flow of blood from a divided artery in asphyxia had ceased, by permitting the usual supply of air to the lung, the circulation was immediately re-established with its usual vigour. Without the supply of oxygen the blood undergoes the asphyxial degeneration; and the circulation being arrested, we must conceive that the vessels destined for the reception of arterial blood are incapable of conveying blood which has undergone this morbid change. But when oxygen is again supplied to the pulmonary cells, the blood becomes arterialized, and at the same moment the circulation is re-established, the minute vessels acting as usual, when stimulated by arterial blood. The celebrated experiment of Hooke is of a similar nature with that of Bichat. It consists in reviving animals apparently dead from asphyxia, by insufflating air into the lungs, until the circulation and natural respiratory motions are restored.* Haller, in whose day the connection of chemical laws with animal physiology was little understood, attempted to explain these phenomena, simply by the mechanical effect of the air upon the lung. "Sed pulmo per suas causas immeabilis, sanguinem ad ventriculum cordis sinistrum nullum transmittebat. Is idem pulmo, per aerem vi impulsum, extensis vasis, diductis vesiculis, sanguini pervius redditur, ut ex dextro corde, ad sinistrum venire possit."

I endeavoured to ascertain the relation of the arterial and asphyxiated blood to the lung in the following manner:

Exp. 4.—Two rabbits being secured, the trachea of the first was tied, and the chest immediately opened. About the third minute in general no pulse could be discovered in the aorta. The left venous sinus was divided, and the blood contained escaped. Small quantities collected to the fifth minute, or even the seventh. The lungs never collapsed. The pulmonary artery being tied close to the root, the beak of the syringe was introduced into its trunk. The vena cava of the same rabbit was divided, and asphyxial blood received into the syringe and injected into the pulmonary artery. I was once or twice deceived in the result. When not injected in quantity and with force, it did not permeate the lung, though immediately afterwards arterial blood obtained from the aorta of the second rabbit penetrated it easily, and was discharged in quantity. But on repeating the experiment, using large quantities of blood, and injecting with greater force, the asphyxial blood permeated, and was discharged at the opening made in the venous sinus. Though this experiment cannot therefore be adduced in favour of the opinion which has been entertained, concerning the chemical relations of the blood to the healthy action of the minute vessels of the pulmonary tissue and its circulation, yet it cannot be adduced against it, the power of operating being so very different, the strength of the human arm, with the mechanical aid of a syringe, containing a much greater quantity of blood, being contrasted with the power of the right heart of a rabbit, acting with little energy in comparison, and on an extremely small quantity of blood.

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* El. Phys. Vol. iii. p. 250.

This part of the subject is much illustrated by the experiments of Nysten. Bichat had supposed that air injected into the veins of animals was fatal by its noxious influence on the brain; and, carried onward by the impetuosity of his sanguine mind, he fancied that he had seen spumous blood filling the carotids and arterial system.

His experiments were carefully repeated by Nysten* with a totally opposite result. No air was ever found in the left heart or arterial system; but the right heart was always distended with a spumous bloody fluid, or with air. Nysten concludes from this appearance, that the injection of air into the veins is fatal, by producing distention of the heart, and mechanically resisting its contraction. He supports this opinion by experiments, showing, that when the spumous fluid is evacuated from the heart, by large openings made in the veins of the neck, the circulation is restored. I think, however, his own experiments contain the evidence, upon which a different explication must be founded. When air is injected in small quantities death does not ensue; and, allowing some interval of time to elapse between each injection, he found it possible to convey into the circulating system of animals, without immediate death, much larger quantities than would produce that catastrophe, were the whole administered at one time. Two circumstances of great importance, however, succeeded those experiments. After repeated injections of air into the veins the arterial blood assumed a darker hue. This effect in some measure depends on the absorption of a greater quantity of azote than oxygen in the dissolution of the air; but much must also be attributed to the embarrassment of the respiratory function, from the air present in the pulmonary blood-vessels.

When, however, repeated injections of air were performed at intervals, death did not ensue immediately. The injections were attended by some rapid and short respiratory motions indicating the pulmonary obstruction. These symptoms seemed in a great measure to vanish, when shortly after the animal coughed, which was the first symptom of fatal pneumonia, which in every instance rapidly terminated its life. These circumstances all tend to prove that repeated moderate injections of air into the venous system are always fatal, by occasioning great obstruction to the pulmonary circulation and consequent inflammation. When exceedingly small quantities are injected, and the animal is left without a repetition of the experiment to the resources of his system, a temporary embarrassment ensues, and the animal gradually recovers. Nysten supposes that the gases have then been dissolved in the blood, and either been exhaled from the lungs, or passed into the general circulation. But when repeated injections are attended with a fatal pneumonia, he supposes, that, all the gases not being dissolved, the spumous fluid obstructs some of the pulmonary vessels, and excites inflammation. It seems therefore strange that he should not have regarded the lungs as that part of the system in which the obstacle to the circulation arose, when large quantities of air were injected. This mode of death is thus reduced to asphyxia, where the circulation, being arrested in the lungs, exactly similar phenomena ensue, from the interruption of the supply of blood to the various tissues. Manifestly, if small quantities of air have such fatal effects upon the pulmonary organs, we cannot conceive that much larger quantities would be able to traverse them.

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* Recherches de Physiologie et de Chimie Pathologiques.

The organic sensibilities of the pulmonary vessels appear also to be illustrated by the fœtal circulation.

If respiration were not necessary to the transition of blood through the pulmonary tissue, it would be difficult to conceive why the *foramen ovale* and *ductus arteriosus* should form separate, less perfect, and unnatural channels for the circulation. Why must all the contingencies attending the dilatation of the lung be hazarded? Why, if there be no obstruction during the absence of the air from the pulmonary cells, if the blood might circulate as freely in the fœtal as in the adult lungs, is it not transmitted through that medium, which, since it is longest employed, must be the best? In the adult, however, the deprivation of air is followed by a sudden arrest of the circulation. When oxygen is again supplied to the pulmonary cells the pulse and life return. These phenomena indicate surely that the circulation is arrested in the lungs.

Goodwyn supposed that the action of the lung continued a longer period than the contractile power of the left heart; and that in asphyxia the circulation was interrupted, because the blood, permeating the capillary system and the lungs, became at length so far disoxygenated that it was incapable of stimulating the left auricle, and stagnated in its cavity. We have however seen, that the action of the left heart continues long after blood has ceased to flow from a puncture made in the pulmonary veins. The experiments of Bichat also prove that the heart circulates dark blood for a short period. He injected dark blood into the left cavities of the heart without destroying their contractility, whilst respiration proceeded with the right lung, and the heart contracted vigorously. * "I † have also re-established," he says "the contractions of the heart, weakened in different violent deaths, by the contact of the dark blood injected into the left ventricle and auricle from a syringe adapted to one of the pulmonary veins." It cannot, therefore, destroy the contractility. The phenomena of the cessation of motion in the left heart in common asphyxia are these. A smaller quantity of blood is received into its cavities, and expelled for a time vigorously into the arteries, the ventricle meanwhile diminishes in size, as the quantity of blood supplied becomes less, until at length, though spontaneous contractions still occur in its fibres, no blood issues from a divided artery, and the ventricle, by contraction, has obliterated its cavity. After this, blood slowly accumulates in the auricle from the large vessels of the lungs, and its contractility continues for a very considerable period. The obliteration of the cavity of the ventricle is especially evident in frogs and cold-blooded animals, where it becomes transparent. The contractility of the auricle, it must be remarked, continues, though it contains the blood which the hypothesis of Goodwyn supposes to be fatal to it. By reference to experiments in a more advanced part of this inquiry, it will be found, that the spontaneous contractions and motive power of the different parts of the heart continue, always in proportion longer as they contain more of the asphyxial blood. Thus, in the experiment of Haller the left heart may be made to contract longer than the right, by retaining the dark blood in its cavity, and by discharging it by an incision made in the venæ cavæ from the right. The action of the auricle may even be restored to its former vigour, by re-establishing the circulation through the lungs, as in the experiment of Hooke, though evidently the insufflation of air into the lungs could not reoxygenate the dark blood contained in the auricle.

Bichat, however, observing that the circulation of dark blood continues for a period, and reasoning from thence that it must pervade all the tissues of the body, conceived that the functions

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* Op. Cit. p. 215.

† Ib. p. 216.

of the various organs were destroyed, by the positive noxious qualities of the blood which circulated in their vessels. This theory, however, as far as it concerns the heart, is supported by no direct evidence. He relates no experiment by which the noxious influence of the dark blood on the heart is demonstrated. The theory principally rests on general arguments drawn from his experiments on other organs of the body. No analogy can, however, be drawn between the heart and other organs, excepting the muscles. The brain, upon which Bichat's most remarkable experiments were performed, has so little analogy with muscle, that, even were it allowed that his experiments conclusively prove the noxious influence of the dark blood upon the tissue of the brain, it would be impossible to infer that its influence on the muscles must be similar. The only direct evidence which Bichat adduces in support of his opinion is the supposed influence of the dark blood on the muscles of voluntary motion. * "Inject into the crural artery of an animal this species of blood obtained from one of its veins, you will perceive the muscles soon becoming very sensibly feebler, and that sometimes immediate paralysis ensues." He then adds, that the ligature of the artery paralyses the limbs, especially if it be placed upon the aorta; but does not attempt to show that the paralysis occurs earlier when dark blood is injected than when the artery is simply tied. The following experiment, however, proves that the effect is entirely owing to the ligature of the artery.

Exp. 5.—Three rabbits were secured. The abdomen of each was opened. The aorta of one was tied above the renal arteries, and also the left common iliac close to the aorta. In eight minutes the vena cava of the second was opened; the blood which immediately flowed was injected into the aorta of the first. The contractility of both extremities was found to be equally vigorous. At the fourteenth minute, the degree of contractility having been ascertained by the insertion of needles attached to the wires of a battery, another quantity of blood was injected. No difference could, by careful examination, be discovered in the contractility of the muscles in the two extremities. The injection of venous blood obtained from the cava of the third rabbit was twice more repeated in the course of thirty minutes. At each interval between the injections, the wires were equally applied to the muscles of both extremities, their contractility gradually and equally declined in each, as in experiments in which the artery is simply tied. Some feeble contractile power survived one hour. Wishing, however, to compare the effect in the muscles, and with the blood of the same

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* Op. Cit. p. 278.

animal, *cæteris paribus*, the following experiment was performed. Present Dr N.

Exp. 6.—If the blood of asphyxia have a more hurtful influence on the functions of muscle than the defect of the arterial blood, if we totally cut off the supply of blood to any muscle in asphyxia, its contractility ought to continue longer than all the others which are subjected to the influence of this fluid. The trachea of a rabbit was laid bare, a ligature was passed beneath it, and one also beneath the abdominal aorta, above the superior mesenteric artery. The aorta and trachea were tied nearly at the same moment. The muscles continued equally vigorous for some time on the application of the wires of the battery. They then equally and gradually declined in strength, and in one hour ceased, contracting to the stimulus at the same time, the muscle of the upper extremity appearing towards the close of the experiment to be somewhat stronger. This experiment was repeated. I afterwards varied it thus.

Exp. 7.—The trachea of one rabbit was tied. In another the chest was opened, and in one minute the heart tied at its base. The muscles continued contractile an equal time in both.

The supply of dark blood from the left heart in asphyxia has therefore no positive noxious influence on the muscles; but they are affected in exactly the same degree as when the supply of blood to their tissue is interrupted.

The connection between the blood and the contractility of muscular fibre is most intimately involved in this inquiry. Blood is necessary to muscular contraction. The most superficial observer is at once impressed with the extreme vascularity and the deep red colour of its tissue when contrasted with that of the rest of the body. Its power is in the direct ratio of its vascularity. All diseases which diminish the quantity of blood impair its power, and most of the causes which augment it are accompanied with a very evident increase of vascular energy. The colour in the young is scarlet, in manhood of a deeper red, and in old age it becomes a yellow and duller red.* It is influenced by sex, temperament, and diseases; is diminished in dropsy, consumption, hæmorrhagy, and in paralysis, the muscles often become pale and discoloured like cellular tissue. Bichat notices † that in many diseases of great prostration of strength the muscles become pallid; motion increases vascularity and power. " Some birds, as the black game, ‡ thus have the external pectoral muscles of a deep colour, whilst the interior are pale." This appearance is observed in the most actively employed muscles of men of various trades. Fowler remarked * that the contractions of a frog's leg were rendered much more vivid by exciting the vascular action.

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* Dic. des Sciences Medical, T. xxxiv. p. 562.

† Anat. Gen. T. iii. p. 333.

‡ Phil. Trans. Carlisle, C. L. 1805.

Boerhaave, Stenon, Cowper, and many other of the older authors, knew that a ligature of the artery destroyed the contractile power of muscle. Legallois, whose experiments in pursuit of other objects often led him to observe the same fact, conceived it merely to prove how necessary the circulation was to the nervous power. Etchepare † has lately more minutely observed its consequences. It must however be premised, that it is impossible to isolate any portion of the body by the ligature of large trunks, since the circulation in the capillary system continues, and with such vigour as to be able to supply nutrition to the part, until, by the increased size and activity of its vessels, it has again connected itself directly with the heart's influence. On ligature of the abdominal aorta in a dog, excessive weakness of the lower extremities ensued, and in eight or ten minutes the motions became almost imperceptible. I have witnessed these phenomena. The contractility is however perceptible by the stimulus of a powerful battery, from forty minutes to a somewhat longer period than one hour, though of course it continues but a very short time vigorous, and towards the close of the experiment the contractions are almost imperceptible.

It is remarkable, however, that though the presence of venous blood in the tissue of muscle has been considered by Bichat and others to be fatal to its contractility, when the vein only is tied, or the artery and vein together, preventing the egress of this blood, the phenomenon does not so soon cease as in the simple ligature of the artery. The power of muscle does not become instantly extinct on the ligature of an arterial trunk, I imagine, because its capillary system still contains a great quantity, and the ligature only prevents the supply of blood from the arterial trunks which irrigate this system, whilst the communication with the whole capillary system, which contains a much greater quantity of blood than the arterial, remains unobstructed. In purpura and scurvy the blood does not coagulate after death, and these diseases are marked by great prostration of strength. In animals which have not the power of quick motion, ‡ the arteries supplying muscles are subdivided into a net-work of extremely numerous small branches, which again unite, retarding the progress of the blood by this mechanical arrangement. From these facts Sir A. Carlisle concludes, that, for the rapid and frequent motions of any muscle, it is necessary that it should enjoy an ample and unobstructed supply of

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* Animal Electricity, p. 128. † Magendie's Journal de Phys.

‡ Home's Comp. Anat. Carlisle, Croonian Lecture, 1800, 1805.

blood. Frequently also in diseases of the arteries muscular debility, or from their obstruction, paralysis ensue. The subject is so extensive, however, that it is impossible to give a detailed view of it in this place, and I shall refer to the sources of information.* I have also in my possession the aorta and obliterated femoral artery of a case of this nature, in which the paralysis was perfect.† The power of contraction is therefore in the direct ratio of the quantity of blood circulating in its tissue, and its capacity is determined by the facility and velocity of that supply. When it is abstracted contractility ceases. Blood is therefore necessary to contraction.

Having from the preceding reasoning formed this conclusion, I inquired whether, after the contractility of muscle had expired from the ligature of an artery, it could be revived by the transfusion of blood, and whether of arterial only or of venous also.

Exp. 8.—A large rabbit and two small ones were confined. The chest of the first was opened on the left side, the aorta divided, and the beak of the syringe introduced into the lower portion. After waiting some time, it was found by repeated application of the wires, that the contractility was extremely feeble in the lower extremities. The chests of the small rabbits were opened, and ere this could be performed the contractility of the inferior limbs was found to have expired. The aortæ of the small rabbits were successively divided; the blood received into the syringe and injected into the aorta of the large rabbits. On applying the needles of the different poles of the battery to the muscles of the lower extremities, their contractility was found to have revived to the degree in which it was found after dividing the aorta of the animal.—Present Dr N.

Exp. 9.—In this experiment a large dog was secured. The arterial blood was obtained from his carotid, and the abdominal aorta tied. After the contractility had expired a considerably longer period than in the last experiment, the arterial blood was received into a warm syringe, and injected into the abdominal aorta whilst the animal was still breathing. The contractility of the muscles of the lower extremities revived. They contracted vividly a few times, and then became motionless. The blood of the dog coagulates so soon, that from this reason probably the power of motion expired earlier than in the other cases.—Present Dr N.

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* Trans. of a Society for the Improvement of Medical and Surgical Knowledge, Vol iii. p. 448. The Medical and Surgical Journal, Vol. xv. p. 14 to 16. Phil. Trans. Vol. xxiii. p. 1194. Granger's Medical and Surgical Remarks, p. 182.

† Edinburgh Medical and Surgical Journal, Vol. xvi. p. 308.

Exp. 10.—This experiment was performed on rabbits. The blood being taken from the abdominal aorta of one, and injected into that of the other, when the motive power was no longer perceptible, by repeated application of the wires of a galvanic battery in vivid excitation, the contractile power immediately revived, and continued vivid for some time. Similar experiments having been afterwards performed with blood of a different character confirming this result, I did not repeat these experiments oftener, especially as to the most eager adventurer in physiology, there is much that is deeply repugnant to the feelings in its pursuits.

Arterial blood is more favourable to contraction than venous. Some physiologists have even asserted that it is essential. Buffon found that newly born animals have the faculty of living a longer time submersed than adults. Edwards, in a work of the most laborious investigation and important results, discovered that this is intimately connected with the faculty of generating heat, * young animals producing less than adults; and that the period during which life continues in asphyxia is intimately connected with this law in animals of the same and different species, and in animals of the same and different ages. Thus hybernating and cold-blooded animals, at certain seasons of the year, may be said to have no proper heat, but assume the temperature of the atmosphere. The faculties of generating heat are *less* than in animals of warm blood; and at the period when least heat is produced, in winter, they live submersed much longer than animals of warm blood. In young mammiferous animals the faculty of living without respiration is intimately connected with this law, and they may be divided into two classes: 1. Those which produce so little heat that they may be said to have no proper temperature, but are influenced by that of external agents. 2. Those which generate sufficient heat to preserve an elevated temperature when the air is not too cold.

The first live long in asphyxia, the others a shorter space of time. In the submersion of these young animals, however, the circulation proceeds; and as the lungs have not been dilated by respiration, it must resemble that of the *fœtus*, the *foramen ovale* and *ductus arteriosus* remaining open. Buffon even thought, that by repeated submersion these channels might be caused to remain pervious, and that thus the animals would preserve the faculty of living a long period without respiration. The phenomenon, however, (were the result of the experiment, as it respects the channel of fœtal circulation, successful,) could not ensue, as the faculty of generating heat increases with the age of the animal, and it requires a greater supply of oxygen. One just conclusion, however, Edwards has, I think, neglected to draw from his experiments. Inquiring whether the blood in this state tends to support life in mammiferous animals, he asphyxiated them by submersion, but arrested the circulation by an excision of the heart in some. Those, in which the circulation was arrested, lived only half the period during which the others survived.

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* Sur l'Action des Agens sur la Vie.

The same laws apply to the cold-blooded animals. In summer, and when exposed to high temperatures, they produce generally a greater quantity of heat, and lose the faculty of living long without respiration. But when their temperature declines under the influence of continued cold, their faculty of generating heat is diminished. I have repeated the experiments in which Edwards discovered that cold-blooded animals can live respiring only the air contained in water. When they have been long exposed to a very low temperature, and water is at zero, they live submersed in water without oxygen two or three days. Their lungs being only an appendage of the general system, the circulation is not obstructed in them when they do not breathe, but continues as before. But if, *cæteris paribus*, they be thus contrasted, if the circulation be suppressed in some by the excision of the heart and bulb of the aorta, and others be exposed to similar circumstances with no supply of oxygen, but without excision; those, in which the circulation continues, live thrice the period that those, in which it is suppressed, survive. He concludes justly, that the asphyxial blood in these animals tends to support life.

But in the young animals, were the *foramen ovale* and *ductus arteriosus* closed, the circulation could not proceed, since the lungs, when they have not been dilated by atmospheric air, are not permeated by the blood of the general system, and after respiratory motions have been established, the circulation is arrested immediately after the loss of the usual supply of oxygen. The life of these animals is prolonged, therefore, because the lungs in asphyxia do not form in them an obstacle to the circulation of the blood, though the period to which this life extends is increased by their low faculty of generating heat.

We have remarked, that in these instances asphyxial blood certainly tends to support life. Its relation to muscle must be now minutely examined.

In former experiments, we have observed that the ligature of the aorta produces paralysis in various periods of time, seldom or never exceeding forty minutes or one hour; but the ligature of a vein occasions paralysis after some hours only. I have often performed experiments, of which the following is an example.

Exp. 11.—In a full grown rabbit, the common iliac artery of one side, and the common iliac vein of the other, were tied. A galvanic battery of forty plates was used to stimulate the muscles of the extremities, the integuments of which were removed from an equal space. The wires of the battery were applied every quarter of a minute to each extremity, for seven minutes, when the contractions became weak. In about a quarter of an hour they were imperceptible in the limb in which the artery was tied, and ceased shortly afterwards, probably because the battery was very powerful, and they were constantly stimulated, though both limbs were equally galvanized. The contractions in the other appeared to be under the influence of the will. The contractility continued, though somewhat feebler, but in three hours the change had not proceeded rapidly; the contractions were still energetic. The cessation of contractile power ensues earlier when both the artery and vein are tied, but not so soon as when the artery alone.

Dark blood is therefore less favourable than arterial to the contractility of muscle, but its presence in the tissue supports this power a considerably longer period than when the artery is simply tied. That venous blood is favourable to muscular contraction is confirmed by the following experiments.

Exp. 12.—The abdomens of two rabbits were opened, and the abdominal aorta having been tied above the renal arteries, the beak of the syringe was introduced into the lower portion. The common iliac artery of the left side was then secured. The motive power continued strong to the stimulus of the battery half an hour. I then observed that the vena cava was extremely distended, and remembering M'Fadyen's experiments on the motion of the blood in the veins, I aided its ascent by constantly drawing my finger along the vessel towards the heart, and then preventing its return by pressure, and performing the same action with the other hand. After having continued this about three minutes, during which time a very considerable quantity of blood had been propelled towards the heart, I was surprised to find that the muscles, which had before been very energetic, had almost lost their contractility—the feeblest and most tremulous motions only being performed. Immediately afterwards, no contraction could be obtained from repeated applications of the wires. The vena cava of the second rabbit was divided, the blood received into the syringe, and injected into the aorta of the first. The contractile power of the right leg, in which the artery was pervious, immediately became strong; that of the left limb, in which the iliac artery was tied, remained extinct. In a few minutes more, a second quantity of blood, which had collected from the divided cava, was injected, and in a few minutes a third. Vivid contractions ensued, on the application of the stimulus in the right limb, and continued, though with decreasing force, a quarter of an hour.—Present Drs N. and B.

Exp. 13.—The experiment was repeated on two smaller rabbits. The aorta and left iliac of the first were tied. In half an hour the contractility of its inferior extremities became weak, and then suddenly expired. Care was taken by repeated applications of the needles attached to the wires, to prevent any motion remaining unobserved. A small quantity of blood only, from the size of the animal, could be obtained by the syringe from the cava of the other. It was injected, and the contractility of the right leg, in which the artery was pervious, immediately and remarkably revived on applying the stimulus; the other leg remained perfectly immotile. The injection was repeated with increased effect. The experiment could not, however, be prosecuted any further from the want of blood, and the motive power soon expired.—Present, Drs N. and B.

I had observed, that after the ligature of an artery, the congestion of blood in the veins of the muscle maintains its contractility a longer period than when the artery is simply tied. I then inquired whether the asphyxial blood had the same power. In experiments, I had found that the asphyxial blood, when freely supplied by the natural contractions of the heart to the muscles of voluntary motion, had no positively hurtful influence. The contractile power of such muscles continuedas long as in those to which the supply of this blood was prevented. But the congestion of venous blood appears to have the power of maintaining contractility a certain period. Does the blood of asphyxia maintain this phenomenon of muscles in a similar way? I observed that the muscles of the superior and inferior extremities were equally congested, from the accumulation of blood from the general system of minute vessels in all the structures of the organization, the inferior with venous, in the sixth experiment, and the superior with asphyxial blood, and the contractility remained during the same period in each. Was not, I inquired, the contractile power of the superior extremities maintained by the congestion of asphyxial blood in their veins?

Exp. 14.—The abdominal aorta and vena cava, and the trachea of a rabbit, were tied at the same time. The chest being opened, the lungs were found well dilated. After a few minutes had expired the superior cava was divided. The needles attached to the wires of the battery were then applied to the muscles of the upper and lower extremities; they were equally and vigorously contractile. From the superior cava flowed a great quantity of blood, which had the asphyxial characters. The muscles of the upper extremities became gradually feebler, and their contractility expired in thirty-five minutes. Those of the lower extremities were very vigorous, when the contractility of the superior was extinct, and continued so during twenty minutes afterwards; and contractions could be obtained by the application of the wires, at the expiration of one hour and a quarter.

These experiments appear to prove, that neither asphyxial nor venous blood have any noxious influence upon the contractility of the voluntary muscles, but that their presence supports the contractile power for a considerable period. The analogy which therefore Bichat has drawn from the influence of venous blood upon the voluntary muscles is futile.

The heart, it may be objected, has a peculiar sensibility; it differs from the voluntary muscles in having a peculiar innervation—in its constant action—somewhat in the distribution of its vessels—and the right auricle is regarded by physiologists as the *ultimum moriens.* These circumstances are sufficient to throw on any analogy drawn from the muscles of voluntary motion a doubt, which, in the investigation of truth, it is necessary to remove. The fact, that the right auricle continues longer motile than any other muscle, has been regarded since the days of Galen as evidence of some peculiarity in the contractile power of the fibres of the heart. It has been generally esteemed to prove that the heart has, in the language of the day, a greater proportion of vital energy, or in that of Haller, a greater irritability.* For instance, Orfila,† commenting on the opinions of Bichat, says, "the heart which is not so soon paralyzed as the other muscles continues still some time to propel the venous blood to all the structures." The continuance of the contractility of the right auricle, after all other muscles have ceased to act, appears to favour this opinion. Nysten and Haller‡ agree in their observations on this subject. The right ventricle and auricle in common death always contracting longer than their corresponding systemic cavities, and the venous sinus and auricle longer than the ventricle. In the experiments of Nysten the action of the ventricles expired before that of the voluntary muscles. The dependence of the muscular contractility on the proper supply of blood has been previously shown, and it is probable that the continuance of contractility, after the obstruction of the great vessels, depends on the general anastomoses of small branches, and the uninterrupted communication of the capillary system, which maintain a supply sufficient to support a feeble contraction. Now it is evident, that, from the imperfect connections of the heart, with the general capillary system, by means of continuous tissue, this communication must be more imperfect in this than in any other organ; and that, consequently, its contractility, unless supported by some other means, ought first to expire. But the right auricle ceases last to contract. The contractility of different parts of the heart expires at different periods.

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* El. Phys. p. 481, Vol. i.

† Dict. de Med. p. 75. Asphyxie.

‡ El. Phys. Tom. i. p. 423, 24, 25, 26.

Have these phenomena any connection with the state of its vascular system, or by what other means is the contractility of the right side of the heart supported longer than that of the left? The continuance of the contractility of each auricle beyond that of its ventricle may perhaps be attributed to the difference of tissue between an auricle and a ventricle; either to the actual difference of the muscular or of the nervous system in an auricle, which, exposed to the same causes, continues to act longer than a ventricle. But we find that the right auricle continues its contractions longer than the left, and the right ventricle than the left, and the reason of this difference is the object of the inquiry. Haller supposed that the motion of the heart was maintained for a longer period, because the natural stimulus which excites the motion of its cavities was present in them to prevent their quiescence. The heart, however, being a muscle, must be subjected to the general laws of muscular fibre. The muscle in life is fatigued by exertion, and after the ligature of an artery, or when the circulation of the blood has ceased, stimuli weaken the remaining force of the contractile power, which expires earlier in proportion as the iteration of the stimulus is more rapid, and as its power is greater. When the supply of blood to the coronary arteries has ceased, each successive application of stimuli must tend therefore to weaken the power of the heart's contraction. The deduction therefore from the following celebrated experiment of Haller must be different from that which he maintained. "I* divided the *venæ cavæ*, and emptied the right auricle and ventricle, afterwards tying the veins. I opened also the pulmonary artery by an ample wound, that the right ventricle might more easily discharge its blood, so that the right auricle and ventricle evacuated their contents; on the other hand, I left the pulmonary veins free, but placed a ligature on the aorta, so that the left auricle and ventricle received blood, but could not expel it. The left auricle and ventricle always continued acting longer than their corresponding cavities, even to the fourth hour. The right auricle was in perpetual quiescence. The right ventricle contracted either imperfectly, languidly, or a shorter time, when it was not altogether immotile, or its quiescence was perfect, as often as the experiment had so succeeded, that the cavities were entirely evacuated.

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* 492, El. Phys. Vol. i.

From this experiment Haller infers, that the presence of the blood in the cavities of the heart maintains by its constant stimulus the contractility of the fibre. But the left auricle always contains a quantity of blood in common death, sufficient, in the language of this hypothesis, to act as a stimulus in maintaining its contractility ; yet it always expires earlier than that of the right auricle.

The congestion of the venous blood in the muscular fibre maintains, during a certain period after the supply of blood has ceased, the contractile power of muscle. The heart, in the death of the brain and lungs, is similarly situated with a muscle whose artery is tied. Blood for a certain time is supplied from the pulmonary veins, and during a certain time its contractions continue as strong as they usually are. As this blood diminishes in quantity, the supply to the coronary arteries becomes less, and the contractions of the heart are impaired. After, however, all supply to the arteries has ceased, the heart continues to contract; but it rapidly becomes feebler, as it now depends for the maintenance of its contractility solely on the blood contained in its fibre. So far the heart obeys the law of muscular fibre in the intimate connection of this phenomenon with the supply of blood. If then, by any means, the egress of blood from its veins were obstructed, this must tend to support its contractility in this state.

Vieussens first, and afterwards Thebesius, Lancisi, Lieutaud, and many other later authors, have described veins of moderate size which enter into the cavities of the right venous sinus and auricle, especially. Haller * confirms their testimony, and describes one small vein whose mouth is generally discovered near the *fossa ovalis*, which returns blood from the right auricle and its sinus. Other similar terminations of conspicuous veins are found in the right sinus and auricle, and, according to Vieussens, even in the *vena cava*. Afterwards in a similar way he describes the venous terminations in the left auricle, its sinus, even in the pulmonary vein. Supported by the same authorities, Vieussens, Thebesius, Rochet, Verheyen, Lancisi, Kawe Boerhaave, &c. other smaller veins have been discovered opening into all the cavities of the heart, and into the sinuses of the *venæ cavæ* and pulmonary veins, with oblique mouths which are protected by valves. Haller informs us that if the tube of an injecting syringe be introduced into any vein of the heart, and all the mouths of the smaller veins that can be discovered opening into the cavities be tied, any coloured liquid or mercury may be injected easily, and will be observed to exude through the oblique terminations of the veins, in the auricles, sinuses and ventricles, but chiefly in the *septum cordis*, and in the right cavities. The *arteria profunda* of the *septum cordis* has no *vena socia*, and it is probable that some other way has been prepared by nature by which the blood of this artery may return to the general circulation ; and Haller insists upon the necessity of its return to the cavities, by veins similar to those described. The deep-seated veins of the muscular fibres of the heart must, it is evident, have more direct communication with its cavities than the superficial, since, in passing through an interlaced tissue alternately contracted and relaxed, their circulation would be nearly obstructed, and the functions of that part of the tissue would suffer in proportion. In the experiment of Haller, quoted above, the permanence of contractility was transferred from the right to the left cavities, by emptying the former, and producing a congestion of blood in the latter. This congestion, which maintained the contractile power of the left cavities of the heart, must have impeded the return of blood by the small veins which enter them, and produced in the fibre that venous turgescence which, when the supply of blood is obstructed, supports contractility. In the cessation of the heart's action from that of the brain or lung, the left ventricle is found almost empty, and its contractility first expires ; the right ventricle, which must be contrasted with it, is gorged, and its contractions continue a much longer period. When, however, their state is changed, and the left is gorged and the right empty, the phenomena are reversed. The left auricle also in this state contains a considerable quantity of blood, but the right sustains the whole pressure of the venous and capillary system, and its contractility is most permanent ; when the circumstances are reversed, by emptying the right auricle, and maintaining a congestion of the left, the contractile power of the right expires first. Nysten also observed, that the contractility of the pulmonary cavities was greatly impaired by injections of air into the veins ; if the injections were excessive the phenomenon very rapidly ceased. A distention with air, or a spumous fluid, must operate not merely by preventing any congestion of the internal veins, and allowing a free escape of blood contained in the fibre, but, as in paralysis of the bladder from a similar cause, by preventing a supply of blood to its fibre. Smaller injections of air, which are not so soon fatal to the contractility of the pulmonary heart, must act simply in the former way.

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* El. Phys. Vol. i. p. 380.

The preceding arguments led to the following experiments.

The left auricle generally contracts longer than the right ventricle, according to Nysten. Has this any connection with the blood contained in its cavity? To determine this point the following experiment was performed.

Exp. 15.—The chest of a rabbit was opened, and in a minute and a half from the commencement, the lungs had been tied at their base, the heart was contracting rapidly and vigorously. At the fifth minute its contractions occurred irregularly and slowly. At the eighth they were eighty-eight. Tenth—left ventricle much diminished; right gorged; contractions feeble. Twelfth—left auricle has ceased; contractions twenty-four. Fifteenth—contractions twenty; extremely feeble. Eighteenth—left ventricle immotile. Twenty-fifth—fourteen contractions of the right side. In thirty-five minutes the ventricle and auricle were also immotile to common stimuli. The right side was gorged, the left empty.

Exp. 16.—In a full grown rabbit the trachea was tied, and in two minutes the chest had been opened, the lungs tied at their base, and the *venæ cavæ* divided at the distance of one-fourth of an inch from the right auricle. The auricle was evacuated; but as no pressure was made on the ventricle, and the pulmonary circulation was arrested, it remained distended. At the sixth minute the auricles were flaccid—their cavities appeared empty—their contraction had gradually become feebler, and less frequent; at the end of eight minutes they were immotile: the *sinus venosus* of the right auricle contracted, however, a somewhat longer period. The left ventricle gradually diminished in size; its contractions became feebler and less frequent; and at the end of eighteen minutes, when it appeared to have expelled all its blood, it became immotile to common stimuli. The right ventricle contracted vigorously at the end of half an hour, and continued longer. On examination, all the cavities, except the right ventricle, contained no more blood than was sufficient to moisten their parietes, but the right ventricle was distended.

Exp. 17.—The left side of the chest of a rabbit was opened, the pericardium cut, and a ligature having been passed round the aorta and pulmonary artery at their origin, an interval was allowed to elapse, during which the animal breathed well, though rapidly, with the right lung. The ligature was then tied close to the root of the arteries. Respiration proceeded well with the right lung, and continued about a minute and a half, when struggles and violent convulsive gasping ensued, and the animal died exactly as in asphyxia. The struggles had ceased in three minutes and a half. The trachea was tied when all motion had ceased several minutes. The right cavity of the pleura was then opened. The mediastinum was discovered perfect; but in the interval which elapsed ere the trachea was tied, some air had permeated through it into the right pleura, and the lung had in some degree collapsed. At the close of the experiment the pulmonary artery was found gorged with blood, which flowed from an incision in very great quantity. The aorta was nearly empty. The circulation had therefore been arrested in its full career, and the cavities of the heart must be distended with blood. The contractions of the ventricles continued synchronous, and they both contracted the last time at the thirty-fifth minute. The auricles contracted one hour and thirty-five minutes synchronously. The right then contracted most frequently. In one hour and forty-two minutes the left ceased to contract spontaneously, though, doubtless, it was contractile to stimuli. The right auricle contracted spontaneously two hours and ten minutes, and was then contractile. The ventricles were equally distended with blood, and ceased contracting at the same time. The right auricle was more congested than the left, and continued longest contractile, sustaining, as usual, the pressure of the whole venous and capillary system.—Present Dr N.

The result of these experiments is perfectly consonant with the idea, that the contractile power of the parts is supported, through a comparatively longer period, from the repletion of their cavities, and consequent congestion of their fibres with venous blood. This repletion of the right auricle, and of its venous sinus in death, from the cessation of the functions of the brain or lungs, must be more considerable than that of any other muscle of the body. The blood, which by the last contraction of the ventricle is propelled into the arteries, is by them conveyed to the capillaries in the various tissues of the body. The organic motions of these tissues continue for some time, the blood still moves onwards, and is impelled with a certain force from their actions into the general venous system. The veins are everywhere found distended, because the progress is obstructed by the lungs, in which respiration has ceased. The pressure of this power must be divided between the terminations of the large veins in venous radiculæ in all the system, and their opposite terminations in the heart. The power, therefore, will be greater in the auricle than in any other portion of the body; and if congestion support the muscular power, that of the auricle (as experiment proves it in these circumstances to happen) ought to survive longer than any other.

In the following experiment, which is in other respects a repetition of that of Haller, the venous congestion was prevented from affecting the right cavities of the heart, and the result was consonant with the previous reasoning.

Exp. 18.—The chest of a rabbit was opened, the *venæ cavæ* tied, and then divided nearer the heart than the ligatures. The heart contracted vividly for some time, then gradually weakened. In ten minutes spontaneous contractions seemed to have ceased. It continued contractile without irritation to the thirteenth minute. The right ventricle and auricle were immotile to the stimulus of the galvanic battery, at the fifteenth minute flaccid and empty. The stimuli were constantly applied, to ascertain the motive power of the different parts. At the twenty-fourth minute no contractions could be obtained from the left ventricle. At the twenty-eighth, the left *sinus venosus* was observed to contain a considerable quantity of blood, which had gradually accumulated from the large vessels of the lungs. The blood was evacuated by an incision, and speedily all contractility ceased. The voluntary muscles, however, continued vividly contractile at the end of one hour, when the experiment was interrupted.—Present, Dr N.

These experiments are consonant with the idea, that the motive power of the right heart, in all cases in which it has been observed to continue longer than that of the muscles, has been preserved by the greater comparative supply of blood to its fibre, from the congestion of its cavities, and consequent repletion of its small veins.

It would be difficult to determine whether much of this effect may not be owing to imbibition, by the open mouths of the small veins, from the blood contained in the cavities of the heart, thus supplying, by their peculiar action, the muscular tissue with the blood necessary to its contraction. Some experiments which I have performed seem to favour this idea. They have not, however, been so frequently repeated as to deserve mention here.

From these experiments, we may at least conclude, that the heart so far obeys the laws common to muscle, that an analogy, drawn from the influence of blood on the tissue of the muscles of voluntary motion, would be strictly applicable to the laws of the contractility of this organ. The blood of asphyxia, so far, therefore, from having any positively noxious influence on the muscles of voluntary motion, or on the heart, is capable of supporting their motive power for a certain period. By what arguments, therefore, is the celebrated hypothesis of Bichat supported?

Bichat commences his inquiries on this subject, by noticing the theory of Goodwyn, (p. 211.) To this he objects, 1st, That if the dark blood paralysed the left heart by its presence in the ventricle, the action of the heart ought to cease first, and that of the organs to succeed, which is contrary to the fact. 2d, If the trachea of any animal be obstructed, (p. 213.) any artery being divided, the blood which flows gradually assumes a darker colour, and at length becomes as dark as the venous. Notwithstanding this phenomenon, which is very conspicuous, the fluid continues still to jet forth, with an energy equal to that of the florid blood for some time. Therefore the heart acts vigorously with this blood in its cavity. 3d, The result of this experiment is the same, though the atmospheric air be in a great measure abstracted from the lung by means of a syringe. 4th, Venous blood injected into the left auricle and ventricle, by means of a syringe, does not weaken the contractions. He therefore inquires, (p. 217.) "quelles sont donc les causes qui interrompent la circulation dans le cœur à sang rouge, et dans les artéres, lorsque le poumon y envoie du sang noir?" He concludes without any further argument or proof, (p. 217.) "I think that the dark blood acts upon the heart in the same way as on all the other structures, as we shall discover that it influences the brain, the voluntary muscles," (which I have shown to be incorrect,) "the membranes, &c. in a word, all the organs which it penetrates, that is by permeating their tissue, by enfeebling each particular fibre; so that I am well persuaded that if it were possible to propel by the coronary artery, dark blood, whilst the florid circulates as usual, in the aortic auricle and ventricle, the circulation would be almost as soon interrupted as when the blood penetrates the tissue of the heart by the coronary arteries, only after having passed the two cavities of arterial blood."

That the genius of Bichat erred in this conclusion is perhaps sufficiently demonstrated. We have before seen in experiment 3, &c., that the action of the left ventricle continues long enough to expel all the blood from its cavity, and that it contracts long after blood has ceased to flow from the coronary veins. Its contractions, therefore, survive the period when it is possible that the coronary arteries can receive the asphyxial blood. The theory of Bichat supposed the noxious qualities of this blood to be fatal to the heart, and that, from this cause, the arterial circulation ceased; but the heart in asphyxia continues to contract vividly when no blood flows from a divided artery. The circulation is arrested by the suspension of respiration, and the consequent cessation of the functions of the lung, at the moment when Bichat supposed the circulation of dark blood to exercise a fatal influence on the heart, which, notwithstanding, continues its contractions vividly for a considerably longer period. But, we have before shown, that the contractions of the heart become less frequent after the supply of blood to the coronary arteries has ceased, in the same degree as the motive power of voluntary muscles is affected after an artery is tied. Other experiments have shown that this blood is favourable to the contractility of the voluntary muscles, and that its presence in the cavities of the heart maintains, during a certain period, the motive power of its fibres. Similar evidences may be obtained, though somewhat more obscurely, from the experiments of Bichat. In opposing the theory of Goodwyn, (p. 216,) he says, " Enfin il m'est egalement arrivé rétablir les contractions du cœur anéanties dans diverses morts violentes par le contact du sang noir injecté dans le ventricule et l'oreillette à sang rouge avec une seringue adaptée à l'une des veines pulmonaires," evidently by giving a new supply to the coronary arteries. In page 217, the comparison of two opposed passages appears unfavourable to his opinions. He asks, " Quelles sont donc les causes qui *interrompent la circulation dans le cœur à sang rouge et dans les artéres lorsque le poumon y envoie du sang noir ?*" Now, in page 227, he says, " Toujours par consequent c'est dans le poumon que la circulation trouve son principal obstacle." The circulation, therefore, is not arrested in the heart; and since the obstruction is thus said to be in the lungs, the supply of dark blood to the heart cannot ensue, and one passage contradicts the other. Yet he concludes in the answer to the question quoted above, that the noxious qualities of this dark blood destroy the motive power of the heart.

An experiment is also detailed in page 217, which is irreconcilable with the theory. When the dark blood has flowed from a divided artery some time in asphyxia, its impetus gradually becomes weaker, and at length ceases almost entirely; if then the stop-cock obstructing the trachea be opened, and respiration be allowed " il se retablit *bientôt* avec *force.*" If in this experiment the circulation had ceased from the destruction of the motive power of the heart, on account of the noxious influence of the dark blood upon its fibres, the renewed circulation of the lungs on the admission of oxygen could have no *sudden* influence on the fibre whose contractility had been destroyed by a noxious fluid, which had gradually permeated and pervaded its tissue, yet the contractions are described to have been *soon re-established with energy.* This, however, may be readily conceived, since we have seen, that the contractions of the heart cease from the want of a due supply of blood, on account of the obstruction of the pulmonary circulation. When, therefore, as in this experiment, the motive power has almost ceased, if respiration be restored, the circulation through the lungs will be re-established, the supply of blood to the heart recommence, its fibre renew its contractions with vigour. The same arguments apply to the celebrated experiment by Hooke, and to the usual phenomena of resuscitation by the inflation of the lungs.

In the application of the same doctrine to explain the interruption of function in the other organs of the body, Bichat has also drawn similar conclusions from the same premises. Examining his argument in detail, we shall discover that it rests entirely on the supposed influence of the dark blood on the voluntary muscles, and the supposition, that this blood continues to circulate a considerable period, and penetrates all the organs of the body, (p. 265.) We have observed, however, that the arterial circulation ceases in a very short time after respiration has been suspended. Bichat principally supports his opinion by the fact, that the tissues of the body become more livid than in ordinary death, and that dark blood oozes from them when they are divided.

Blood darker than the arterial certainly permeates the lungs, and is transmitted from the left ventricle during a short period to the various organs of the body. But the time during which this blood circulates when the respiration has been suddenly suspended is so short, that we cannot suppose that it has any great influence in producing this colour, especially as the phenomenon is capable of an explication more consonant with the facts detailed in this inquiry. In the variety of asphyxia often observed in diseases of the lungs, especially when complicated with the extreme debility of fever, when the functions of the lungs are gradually impeded, the colour of the blood is changed in all the issues of the body ; but as death does not ensue, until, as in common asphyxia, the circulation has been arrested from cessation of the functions of the lung, this blood has no sudden noxious influence on the various tissues. Hunter, however, discovered, that the colour of the blood in contact with any living tissue, gradually becomes darker, until it has assumed the venous colour. After death, on what lesion soever it may depend, we never discover bright arterial blood in any tissue of the body. The state of the vascular system differs also in asphyxia from that observed in other kinds of death, as the blood is chiefly accumulated in the veins, and the radiculæ and minute vessels terminating in them. That the tissues therefore should assume a somewhat livid colour, is not so peculiar that it must be ascribed to an universal morbid change in the blood, which cannot happen when the respiration is suddenly interrupted ; but it must be attributed to the strange influence of tissue upon blood, by which its qualities assume in a greater degree the venous characters, when surrounded by it, and to the peculiar state of congestion of the venous system, rendering this colour more obvious to the eye.

Bichat proceeds (p. 275,) to attempt to prove " *that the dark blood is incapable of supporting the activity* and the life of the organs which it penetrates, when the functions of the lung have ceased." The arterial blood, he remarks, is peculiarly adapted as a vital stimulus to the various organs of the body; (p. 275–276,) and (p. 277) he speculates concerning the method by which it supports the various organic motions. He then proposes (p. 278) to examine how the contact of the dark-blood with the structures occasions their death. The action of all the organs of what Bichat has termed animal life depends on the brain, and, supporting his opinion by his experiments concerning the influence of the dark-blood on the brain, he supposes that it is through this medium fatal to them. "But when," he adds, "this fluid circulates in the general system, when all the organs are subjected to its influence, two other causes are added to this. 1. The nerves which are penetrated by it are no longer capable of establishing communications between the brain and the senses on the one hand, and on the other, between this viscus, and the locomotive and vocal organs. 2. The contact of the dark-blood with the organs themselves destroys their action." For he asserts, if the dark-blood be injected into the crural artery of an animal paralysis supervenes. He repeats these opinions in page 280. But its influence on the nerves rests simply on the opinion of Bichat, unsupported by experiment, and we have shown that it has no noxious influence on the muscle. Pursuing the argument, he remarks the difficulty of proving satisfactorily "that the secretions, exhalation, and nutrition, cannot obtain from the dark-blood the materials necessary to support them, for this blood does not circulate in the arteries, during a period sufficiently long, to enable us to make experiments on these functions. He then remarks, that, after respiration had been suspended, he had never witnessed the flow of semen from the *vas deferens*, or of urine from the ureter, but he allows that no consequence can be adduced from observations of this nature, made during the short sufferings of the animal. "It is therefore," he continues, "principally from the analogy of that which happens in other organs, that we determine that the functions of secretion, exhalation, and nutrition, cease when the dark blood penetrates their structures." But upon what analogy? The only positive experiments, adduced in support of these opinions, concern the effect of the dark-blood on the muscle and the brain. The first we have shown to be erroneous; and concerning the second we can only at present remark, that, allowing the experiment to be correct, the fact that the dark-blood supports and revives the contractility of muscle demonstrates that no analogy can be drawn between the brain and other organs. Analogy cannot therefore be adduced in aid of this opinion.

The argument of Bichat here ceases; and it is needless to follow him through the subsequent pages, in which he attempts to trace a vague accordance between his opinions and some of the obscure and even doubtful phenomena of asphyxia.

To these opinions it must further be objected, that in young animals, as is proved by the experiments of Buffon and Edwards, and in animals of cold blood, life, with all its functions and phenomena, is supported for a considerable period by the presence of the dark blood; and as the hypothesis of Bichat is supported by no valid experiment or argument, we are inclined to the opinion, that, though the dark blood is doubtless less favourable to the action of the organs generally, than the arterial, yet its presence exerts no positively noxious influence upon them, but is even capable of supporting their actions for a certain period. The evidence of more decided experiment is desirable to confirm this opinion, as far as it respects the organic motions generally. Concerning muscle the experiments detailed in this inquiry are positive; and the blood being necessary to secretion and nutrition, it is a natural consequence, that, when the circulation is arrested, as in asphyxia, these functions should cease.

I embrace this opportunity of expressing my great obligation to my enterprizing colleagues, Drs Nesbitt and Bruce, for their very valuable assistance in some experiments, and for their watchful scrutiny during the progress of almost all.

ART. VI.—*Case of Extra-uterine Fœtation.* By JOHN COOPER, Esq. Surgeon, Liverpool.

MRS H. a tall well-made woman, 25 years of age, called to consult me on Thursday, 26th July (ultimo.) She said she had suffered much during the last two months from repeated attacks of colic, for which, under the care of two respectable practitioners, she had been often bled, blistered, and taken various medicines. At this time she was free from pain of any kind; but her countenance expressed great anxiety. Frequently of late her food had been rejected soon after it was taken; and when not vomited, it generally passed off quickly by the bowels, little changed in appearance, and creating great irritation. She informed me she had been married four years; that she had two children previous to her marriage,—indeed before she was seventeen, but none since, and confessed that she had led an intemperate life. From the weaning of her last child the catamenia had constantly appeared at the regular periods, and in due quantity. She mentioned now a tumour in the right side of her abdomen, and which I found to occupy nearly the whole space between the umbilicus and ilium.

This tumour, circumscribed, firm, and unyielding to the grasp, gave no pain when pressed; nor was there any part of the abdomen tender to the touch. The patient assured me the mammæ had undergone no change since the commencement of her complaints. My inquiries, therefore, rested here for the present; and, confining my attention to the means of improving the state of her stomach and bowels, I prescribed some purgatives and alkaline tonics, and requested to see her again in a few days, trusting that a little time would develope the nature of the tumour, and the cause of the constitutional irritation.

On the Sunday following, July 29th, I received a message, pressing me to visit her immediately, (3 P. M.) Upon entering the house I heard her screaming with pain, and found her up stairs upon the bed laying upon her back, with the knees drawn up, her face excessively pale, and covered with clammy perspiration; her abdomen universally tumid, and extremely tense and tender. The pain, however, seemed to centre in the right hypochondrium, and to extend to the right shoulder. She had had several scanty evacuations by stool both that morning and the previous day. Her urine, too, had been passed frequently in small quantities, and of a dark colour.

From the commencement of this attack of pain her stomach would retain nothing but a few spoonfuls of cold water, and that only for a short time. The pulse was small, hard, and quick.

The case being now obviously acute peritonitis, involving, as I conceived, the coats of the liver, I took away thirty ounces of blood from the arm. A state approaching to deliquium was produced, continued some time, and considerable relief was obtained. Directions were left for the application of fifteen leeches to the abdomen, assiduous fomentation, and the frequent injection of emollient clysters.

At 10 P. M. The tension of the abdomen was much diminished; the pulse somewhat softer, and not so frequent; pain still constant and severe; the stomach was very irritable; and the tongue covered with a slight white fur. The leeches had bled well; but the injections had been neglected. A large blister was ordered to be applied as soon as the cessation of the local bleeding would permit; and some pills containing a grain and a half of opium, with three grains of calomel, were to be taken every four or six hours, according to the urgency of pain.

Monday, 29th, 8 A. M.—The tension and tenderness of the abdomen were much lessened; the pain in the right side and shoulder still severe, and always increased by a deep inspiration. The skin, however, was relaxed, and of a general warmth. The blister had risen; the pills were said to have been vomited; but she nevertheless had dozed occasionally during the night. Some injections had been given, and a small quantity of feculent matter brought away by each. A mixture containing Epsom salt and subcarbonate of magnesia was taken at intervals during the day, but always rejected. By continuing the opium and calomel, however, the stomach became gradually less impatient, and next day, July 30, bore a dose of castor oil with tincture of senna. Copious stools were produced by it, and arrow-root gruel and other light nutriments beginning to be retained, her recovery now went on rapidly. A slight ptyalism was induced by the calomel, however, and soreness of the gums continued for about a week.

August 14th.—Passing near her residence I paid her a visit; she was below stairs, at work with her needle, and very cheerful. No tenderness or pain felt in any part; her appetite good; and she considered herself better than she had been for many months. During my attendance I had particularly noticed the appearances of the mammæ; and from their size, feel, and darkness of the areolæ, and from repeated examination of the abdominal tumour, was convinced that she was pregnant. Of course I declared this conviction both to herself and to her friends. At the same time, reflecting upon the situation of the tumour, and that it seemed more loosely attached than the enlarged uterus could be, I mentioned to my pupil, Mr Lane, who had regularly attended the case with me, the probability, and my suspicion of an extra-uterine fœtation having been the cause of this violent attack of peritonitis.

August 16th.—In the afternoon of this day I received a message desiring me to visit her as soon as possible. An attack in every respect similar to the last, but not so violent, had come on. Fifteen leeches to the abdomen and fomentations were prescribed. Mr Lane saw her for me late in the evening. At his return he reported to me that he found her lying upon the floor in an expiring state. He was informed that a little before he arrived a very remarkable increase in the size of the abdomen having taken place suddenly, Mrs H. in an agony had thrown herself out of bed. As I was engaged Mr Lane returned to wait the event. She expired about twelve the same night.

Permission being obtained, I examined the body next evening, in company with my friend Dr Nicholson, my brother, and Mr Lane. The abdomen appeared extremely tumid, and felt remarkably tense.

Upon reaching the peritonæum, that membrane was found on all its fore part considerably thickened, opaque, and white; and upon slitting it open a stratum of coagulated blood, about half an inch thick, presented in every direction, so that no glimpse of omentum or of intestine could be obtained until this was removed. In the situation where the tumour had been first noticed, that is, in the hypogastric and right iliac regions, the coagulated blood was found in greatest quantity, and intimately mixed and confused with a thickened, spongy, exceedingly vascular, and tender omentum, partially imbedded in which was discovered a fœtus about seven inches in length, with its head and shoulders, however, protruding through the diseased mass. Although the coagula were removed with every care, and the umbilical cord followed as a guide, the placenta was separated from its remaining adhesions before its original attachments could be well observed. The right Fallopian tube, however, was found enlarged to a pouch, burst open, and its inner surface curiously rough, with hanging fibres. In the left ovarium was observed a hydatid about the size of a walnut. The uterus was free from all marks of disease, but its walls were thickened, the *os tincæ* closed, and the organ altogether about twice its usual size in the unimpregnated state.

The peritoneal coats of the cœcum, ascending colon, and of the ileum on the right side were highly vascular, and turgid with dark-coloured blood. These appearances, however, scarcely extended to any parts of the intestinal canal on the left side, whilst the coats of the *liver*, stomach, spleen, kidneys, and bladder seemed to have been exempt from any recent inflammation.

The uterus, with its appendages, having been obtained for a preparation, and wishing to preserve them as little changed by dissection as possible, I did not make a more particular examination of them till to-day, just previous to sending you this communication, when, supposing that more perfect information with regard to these parts might be required, I cut open the uterus by an antero-posterior section. It was found filled with a grumous fluid, and completely lined by the deciduous membrane.

The situation of the entrance of the Fallopian tubes is clearly marked, but the orifices are impervious to a probe or to bristle. *Both tubes* indeed are completely shut up, and consolidated on each side for about an inch.

A perfect ovarium was seen on the left side; but none could be met with on the right.

The patient had been out and taken considerable exercise the day before her death, of which the separation of the placenta, and consequent hemorrhage appear to have been the immediate cause. The diameter of the placenta measured nearly three inches.

Queen Square, Liverpool,
12*th Sept.* 1827.

ART. VII.—*On the Duration of Fatal Cholera.* By Mr EDMUND TATHAM, Surgeon, and Member of the Royal Medical Society of Edinburgh.

THE following case is one such as may occur now and then to medical men, and though in itself perhaps not of much value, yet, at the present time, when some discussion has taken place as to the probably fatal termination of a case of cholera in this country within twelve or twenty-four hours, it may not be altogether out of place. When doubt exists in any case, nothing is so likely to remove it as authenticated facts; and where a statement is given in unison with truth, provided it bear upon the point in question, nothing objectionable can arise out of it.

I was sent for about four o'clock on Friday morning, August 31st, last, to see Anthony Dixon, aged 49, a farmer, of rather spare habit, but who had always enjoyed good health previously. I found him suffering under most severe symptoms of cholera. His extremities were cold; the pulse quick and irregular, scarcely to be felt; countenance cadaverous, and bedewed with a cold perspiration; tongue somewhat furred and dry; the bowels in a most disturbed state, and their liquid contents flowing from him whilst lying.

On inquiring of his wife the particulars of the attack, I obtained the following information: That a little after one that morning she was awakened by her husband getting out of bed to go to the night-table, which he reached with difficulty, and where he evacuated such a quantity of liquid matter as perfectly astonished her; that his strength failed him so much afterwards as to disable him for returning to bed without considerable assistance; that some time after this on her giving him a little rum and water, he vomited, and complained of pain in his bowels, and became so low and exhausted as to lead her to suppose him dying. She showed me what had been dislodged from his stomach, and likewise what had been passed by stool. The former was to appearance a tolerably clear fluid tinged with bile, the latter a slightly brown liquid, mixed with excrementitious matter of most nauseous smell. It was stated that the cramp in his legs had been so painful as to cause him to cry out, and make frequent efforts to rise.

Under such circumstances I thought no time was to be lost in combating a disease so quick in its progress, especially, as in the present case, where the vital powers seemed to have sustained a most serious shock. Accordingly, I requested hot fomentations to be immediately applied to the epigastrium, and bottles of warm water to be placed to the feet, which I further ordered to be well rubbed with warm hands. He took from me a mixture of laudanum, *liquor ammoniæ acetatæ*, and camphor mixture, together with warm brandy and water. In case the vomiting should return, I begged that one of a few pills, each containing two grains of extract of opium, and four grains of calomel, might be taken; and, provided warmth and strength returned, the following powders were directed to be administered every two or three hours:

℞ *Pulv. Hydrarg. cum creta; Pulv. Rhei ā ℈ij.; Pulv. Cinnam. c. ℈j.; Pulv. G. Trag. c. ʒiss. M. N. div. in pulv. viii.*

On leaving him, after remaining about an hour and a half, I ordered the attendants to continue most assiduously the restorative means, till nature seemed to have rallied, when thin gruel and tea might be given him for drink in case of thirst, which had not been troublesome as yet.

On my return at 10 A. M. to see him I found no amendment. The general powers of the system had sunk still lower, the extremities being something colder than before. His bowels were still very open; as cloths, which had been applied in consequence of inability to get up, were almost saturated with moisture. Little pain was felt now in any part, although there had been some both in the bowels and extremities since my visit. The pulse, which was felt with difficulty, was intermitting and tremulous. His breathing was hurried and disturbed, impressing the features with anxiety; and that peculiar contracted cast of countenance which so often accompanies intestinal irritation was distinctly visible. He was merely able to give an answer in the negative to an inquiry made as to pain that might be felt anywhere. Vomiting having taken place again, one of the pills was taken as well as a powder, which remained upon his stomach. The warm remedies, both internal and external, had been assiduously used, but without effect.

Every prospect of a speedy dissolution being evident, I had no idea of being of the least service; and with the exception of giving the poor man a little brandy and laudanum for form's sake, I left him, with desire that quietness might be observed as becoming the occasion. A messenger informed me that life became extinct a little before 1 P. M., making a space of about twelve hours from the apparent beginning of the attack.

That this sudden malady could not have existed long before the time of the patient's getting out of bed, when his wife was disturbed, must be inferred from her statement, that he went to rest in good health, having gone through the business of the farm with more than usual exertion during the day, and with appetite unimpaired partaken of food with the family, and talked in good spirits a short time before retiring, which was between nine and ten, about three hours before appearing so ill. Moreover, he himself signified that he was ill but shortly before awakening his wife.

The symptoms throughout were decidedly those of cholera, and of a sort which we do not often witness in this country,—more like that which so suddenly carries off numbers in the warmer climates. The infrequency of such cases with us has no doubt given rise to an opinion lately advanced by a professor in an elevated school of medicine, that this disease never proceeds to a fatal termination within two days; * an opinion certainly which cannot but have greatly surprised those who have had occasion to notice much of this disorder, as existing here in some autumns, when patients have been reduced to the very lowest condition in the space of twelve or twenty-four hours, that no surprise or astonishment could reasonably have been entertained had death supervened immediately. By an effort of memory, too, instances must occur where medical men have had the pain of seeing individuals snatched from them by the hand of death in the space of twenty-four, thirty-six, or forty-eight hours from the commencement of the attack, in spite of every effort had recourse to; often unfortunately too late, from a desire, especially in the country, to dispense with advice.

To the doctrine that this disease will terminate favourably without the interference of art, few I think will subscribe who have suffered from the anxiety connected with attendance upon constitutions of a delicate structure, where something must be tried, and where evidently benefit has appeared to result from it. As respects the above case, but little opportunity was given for doing much, the extreme state of collapse which came on at once never leaving the patient. In such cases, where the whole frame has sustained as it were a death blow, no art or management can do almost any thing; yet it is impossible not to feel desirous of attempting some means of relief, in hopes that success may attend the effort, ineffectual as it too often is. When the first symptoms consist of great depression and want of free circulation, our efforts perhaps cannot be better exerted than by attempting to equalize the latter, and by restoring to the extremities their wonted warmth and power. For this purpose the usual stimulating and rousing means both internally and externally applied seem most likely to afford successful results. Opium, accompanied with camphor, ammonia, and similar cordials, generally affords agreeable warmth and comfort to the stomach. Indeed, the first is often most felicitous in checking the train of morbid action, and recovering the exhausted frame

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* If the author alludes here to what has been stated in the July Number of this Journal by Dr Christison, he will find on reperusing Dr Christison's paper, that no such opinion has been there advanced.—EDITORS.

from tremor and agitation. According to recent accounts, where considerable congestion oppresses the patient, which in some late forms of the complaint was very great in India, general blood-letting was practised with relief, and on a rational principle; but in this country it has not been much employed, except in a few constitutions. The good effect resulting from the use of leeches to the abdomen, where pain and tenderness exist, must have been apparent to all. If the practice emanating out of the gastro-enteritic doctrine of Broussais has on the continent been frequently misapplied, we cannot, with much truth, accuse ourselves of any such error here. How many cases of inflammation of one or other of the coats of the intestines are allowed to follow their course without that occasional use of local evacuants which, without impairing the general strength of the system, tend so powerfully to abate the force of the disorder.

Calomel, since its first recommendation by Corbyn, the East India physician, has been extensively used in cholera, especially in the warmer climates, and according to the evidence of several with marked benefit. Annesley, whose excellent treatise on this disease adds much to the knowledge previously extant upon it, speaks very favourably of calomel as improving the secretions, and dislodging the concrete mucus from the internal coat of the bowels. A combination of this medicine, with extract of hyoscyamus and opium in moderate doses, given every three or four hours, is frequently prescribed with advantage in this part of the kingdom, if the regular action of the bowels be kept up at the same time by mild aperients, as castor oil, which, when given in a suitable vehicle, sits more easily upon the stomach than might be supposed.

When nausea prevails, and thirst is urgent, saline draughts, with an excess of alkali, are highly grateful to the sufferer, and assist in allaying the irritation of the stomach and bowels, unless the inflammation of their internal membrane be considerable, when but small benefit can be looked for till a copious stock of leeches be applied to the abdomen. By the way, it is curious to notice the quantity of small particles of concrete mucus, having the appearance of organized matter, that are occasionally evacuated after irritation has continued for any length of time. It generally alarms the attendants very much, and requires an explanation. I have seen a favourable change very often follow this discharge. Mustard poultices applied over the epigastrium often act on the principle of counter-irritation in a very satisfactory way; and I have used a bran poultice charged with some laudanum and brandy with a good effect in old persons, who have declared that the comfort they experienced from it was very grateful. The spasms that so frequently add to the patient's distress in cholera denote a good deal as to its nature. When the malady prevailed in this place in the year 1825, and was virulent, cramp was almost a constant symptom, not unfrequently showing itself before the other symptoms appeared to have set in. It may be relieved when in the extremities for a time by friction with warm spirit or *Eau de Cologne;* but it is liable to return and tease the sufferer, even when exhausted to that degree that he scarcely seems susceptible of pain.

Diluents, as barley-water, weak wine-whey, arrow-root boiled in water, with or without milk, in moderate quantities, serve both to refresh the sick person and dilute the irritating contents of the *primæ viæ.* I have not had an opportunity of learning any thing as to the efficacy of copaiba or turpentine when administered internally in this affection. They are extolled for their power in relieving some inflammations of mucous membrane; but their nauseous flavour is a frequent objection to their use. The former of them is represented to be now prepared in such a way as to obviate this inconvenience.

What the cause of cholera may be is little known. Numerous conjectures have been thrown out regarding its origin and formation. Nearly all are in favour, however, of the influence of the atmosphere in its production, when assisted by certain conditions of the constitution and peculiar circumstances. Some imagine that an electrical state of the air affecting the nervous energy, and depressing the vital principle, gives rise to the distressing train of symptoms. Others affirm that terrestrial exhalations produced by solar influence act upon the body as miasmata do in the generation of fever. Others again blame certain *ingesta* introduced into the stomach whilst debilitated by irregularities and vicissitudes of temperature. The liver has not been quietly passed over without partaking of a degree of odium in not rightly secreting the bile, according to the notions of some.

Dissection, in tracing the alteration of structure following a protraction of deranged vital movements, may throw a gleam of light upon this subject, but probably will, as is too commonly the case in *post mortem* examinations, disclose little as to the first principles of diseased action.

Those few remarks spring from a desire to call attention to a subject on which it would be highly desirable to obtain satisfactory and useful information. It does not follow that nothing should be discussed except where novelty can be introduced. When one calls to mind "nihil dictum quod not dictum prius," he may take comfort, though engaged merely in dwelling upon matters of frequent occurrence, and recapitulating things already known.

Kendal, September 8, 1827.

Art. VIII.—*On the Causes of the Fatal Termination of certain Cases of Bronchotomy.* By William Cullen, M. D. F.R.C. Surg., Surgeon to the Royal Infirmary, Lecturer on Anatomy and Physiology, &c. &c.

In the year 1820, the author, when attending as a pupil at St Bartholomews, in London, was called upon to read before the excellent Medical Society of the hospital an essay on bronchotomy, which was subjected to the criticism of the members during a discussion of several nights. Having had his attention thus early directed to the subject, he has ever since been attentive to the progress of medical surgery in what relates to the trachea, and is enabled to reflect, with some satisfaction, that his own observations have not been entirely useless.

It is well known to those acquainted with the history of surgery, that bronchotomy was very rarely practised by the ancients, and only as the *ultimum remedium* in the most desperate cases of inflammatory cynanche.

Rarely practised by the ancients, it had fallen into complete oblivion among the moderns, till M. Louis, in the Memoirs of the French Academy of Surgery, recalled it into notice, explained the circumstances in which it was necessary, and described the best modes of performing it.

But Louis and the other French surgeons, his contemporaries, directed their attention chiefly to the case of foreign bodies accidently dropt into the trachea; while they were comparatively ignorant of the far more numerous class of cases which depend on inflammation of some portion of the air-passages. To this part of the subject the attention of the profession was directed, first by Dr Baillie, and after him by several writers in the earlier volumes of the Transactions of the Medico-Chirurgical Society of London, particularly by Mr Lawrence, Dr Farre, and Mr Chevalier.

By the united researches of the French academicians and our own most intelligent surgeons, it was indisputably proved, that bronchotomy afforded the only chance of relief in the case of foreign bodies in the trachea, and the only chance of recovery in many cases of cynanche laryngea.

It was proved also that wounds of the larynx or trachea, though inflicted on membranous or cartilaginous parts, were not, on that account merely, of a very dangerous nature, as the ancients had erroneously supposed; and the *absolute safety of* the operation was perhaps too confidently maintained. At least, I must confess that I have been induced to form an opposite opinion from reflecting deeply on the uses of the larynx, and on symptoms which I have witnessed in cases that have fallen under my observation.

In illustration of the opinion that wounds of the air passages, even when inflicted on a healthy subject, are not only not trifling, but often highly dangerous, and followed by fatal consequences, the following case, which was treated by me in the Royal Infirmary may be quoted.

* A. B. a healthy-looking married woman, 25 years of age, and mother of one child, was subjected at home to many causes of mental irritation from the misconduct of her husband, and the unkindness of her friends. These acting on an irritable habit produced a great depression of spirits, under the influence of which she tried to lay violent hands on herself by cutting her throat with a table-knife. The instrument, however, was blunt, and she failed in her object, having been able to make only a slight incision of about three inches in length through the skin and cellular tissue over the crico-thyroid membrane. The latter likewise was divided to a small extent, and an opening was made into the larynx hardly capable of admitting the point of the little finger.

The accident occurred at 10 A. M.—When I saw the patient about noon I found the wound in the situation, and of the size above described; she breathed entirely through it, and consequently her voice was lost, except when the orifice was stopped up, when she spoke quite distinctly by means of the glottis. She had no difficulty of breathing; no attempt at coughing; no hemorrhage. In fact, she made no complaint. The case, however, seemed otherwise alarming. She was slightly delirious, always on the subject of her domestic grievance; the pulse was small and feeble; her face was rather flushed; the tongue was white and tremulous when protruded; and the countenance had a peculiar expression of anxiety and suffering. By means of a proper bandage the head was bent down upon the sternum, so as to close the orifice in the crico-thyroid membrane, upon which a simple pledget was applied, no other dressing being thought necessary, as it was determined to let the wound heal slowly, in its own way, by granulation.

The head symptoms seemed much more urgent; inflammation of the arachnoid membrane was suspected, a disease which I have uniformly found most difficult of cure. When depending on mental distress it is a complaint often fatal, particularly when, occurring in those who have attempted their lives, it is treated too actively by blood-letting. I contented myself, therefore, with desiring that the patient should have her head shaved, and that ice should be applied to it; that she should occasionally have a small quantity of wine and water if she seemed disposed to sink; that a brisk cathartic should be given; and that she should be removed to a darkened chamber, where she could be kept cool and perfectly quiet.

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* The dates of this case cannot be given, as the Journal has been purloined from the Royal Infirmary.—W. C.

These measures appeared to have a very salutary effect. At 7 P. M. she was more composed; the expression of the countenance was improved; the dressings were displaced, and she breathed fully from the external orifice. The wine was ordered to be stopped, and the ice to be regularly continued.

The next morning she was reported to have passed a quiet but sleepless night. The pulse was firmer than before. She had some cough, or rather violent expirations in place of cough, unaccompanied by sputum of any kind.

In the evening the delirium seemed to have increased, with tendency to noise and violence. The respiratory symptoms were the same as before. Ten leeches were ordered to be applied to the forehead and temples.

Next morning she was rather better; the cough, however, was somewhat increased, and she had slight expectoration of viscid clear transparent mucus. This expectoration was brought up with great difficulty. It was expelled through the wound, not by coughing, strictly so called, but by expiration merely; and that with so little force, that it had to be pulled out of the wound by the fingers of the patient or the nurse. A mucilaginous antimonial mixture was ordered. In the evening the symptoms were much the same as in the morning; the expectoration rather more copious; and the stethoscope detected acute inflammation in the smaller branches of the bronchi. A large blister was applied to the sternum, as the state of the intellectual functions and constitutional powers seemed to forbid the use of the lancet.

Next morning the patient was thrown into considerable agitation by a visit from her husband. She was very delirious at the visit at noon. The sputum was more copious, and still got rid of with much difficulty. She was ordered an anodyne draught, which procured a few hours quiet repose, and soothed her excited imagination. I was in hopes, therefore, that the patient was to do well, as she had no threatening symptoms referable to the chest, and as the head symptoms were comparatively moderate and manageable.

Next morning, however, at five o'clock, Dr Lubbock, the house-surgeon, to whom I am much indebted for his assistance in the management of the case, was suddenly roused from his bed, as the patient was reported to be dying. He found her on the point of suffocation; breathing very hurriedly with great difficulty; with the death rattle in her throat, livid face and cold extremities. She was unable to expectorate. Dr Lubbock immediately introduced a large curved canula into the wound, through which the patient was enabled to take a full inspiration. The effect of the introduction of the instrument was, that the breathing became much easier; and, as the alarming symptoms evidently arose from the accumulation of mucus in the bronchi, *she was strongly recommended to cough as much as she could.* By degrees she was enabled to bring up mucus through the canula, not, however, by coughing, but simply by expiration, which, as the canula was of large size, she could perform with considerable force.

In the course of a short time, as the expectoration became more free, the alarming symptoms subsided; the rattle in the throat was no longer heard; the breathing became easy; and the expression of the countenance very greatly improved.

Having been thus apparently rescued from the very jaws of death, she was left under the care of the nurse, who was directed to request her from time to time to make attempts at coughing. At noon, when I paid her a visit along with Dr Lubbock, I found her very nearly in the state from which she had been recovered with so much difficulty in the morning. The canula had slipped out of the trachea; the extremities were cold; the lips livid; the pulse hardly perceptible; the respirations rare, difficult, and accompanied with a frightful rattle in the bronchi; no expectoration; no attempt at coughing; and a great degree of stupor. In a loud tone of voice I desired her to cough, *but she could not make the attempt.* To enable her to expectorate, to allow her to cough, I placed my thumb on the orifice of the crico-thyroid membrane, so that she breathed entirely by the glottis much more freely than she had done before through the artificial opening.

When the respiration was a little improved, I requested her to cough. She could now do so readily enough, and brought up some very tenacious mucus, by the excretion of which the respiration was still further improved.

In the course of about a quarter of an hour, in consequence of the free expectoration, the breathing became quite easy; the expression of the countenance improved; and the lips regained nearly their natural hue. I directed a dresser to remain constantly with the patient, to keep the crico-thyroid membrane closed with his thumb, and to direct her to make attempts at coughing from time to time, and, if she refused, to tickle the epiglottis with a feather. For about an hour and a-half the patient complied with the directions given her, and coughed up a considerable quantity of mucus; but by degrees she became more and more insensible, and would not cough; titillation of the epiglottis produced no effect; the mucus accumulated in the bronchi; and she died of suffocation about four o'clock.

Sectio Cadaveris 21 hours after death.—Upon opening the cranium, the vessels were found much more numerous than usual, more highly injected, especially on the surface of the right hemisphere. There was a good deal of serum in the subarachnoid cellular tissue, but the serous surface of the membrane was perfectly natural.

The substance of the brain was more vascular than usual, but not inflamed, softened, or otherwise disorganized. There was a considerable quantity of serum in the interior of the ventricles, a large quantity also at the basis of the brain, where likewise the membranes were much more vascular than usual, particularly on the tuber annulare.

Upon opening the chest the pleura was found in a perfectly natural state.

The bronchi, particularly the left, were obstructed with bloody mucus. There was a small quantity in the trachea. The lungs were everywhere soft, crepitating, elastic, and only a little more vascular than usual.

In the neighbourhood of the wound the mucous membrane of the larynx was inflamed, and red from increased vascularity, but it was not thickened. There was no effusion of fluid into the submucous cellular tissue, and consequently no diminution in the diameter of the glottis. Immediately at the edge of the wound there was a slight effusion of lymph. The membrane of the trachea and that of the bronchi was inflamed and highly vascular, not thickened, but perhaps a little softer than natural; and it seemed as if the inflammation had spread down the air-passages, into the minute branches of the bronchi.

This case illustrates in a very forcible manner the truth of some observations on bronchotomy published in the number of this Journal for July last. I took occasion in that paper to call the attention of the profession to the physiology of coughing, and to the danger which necessarily attends on a copious secretion of mucus from the bronchi when an opening has been made in the trachea. For, to produce full and vigorous coughing, so as to clear the minute branches of the bronchi of their fluid secretion, the expiratory muscles must for a certain time act in opposition to the constrictor muscles of the glottis. When by this antagonism the former have acquired sufficient force, the latter are suddenly relaxed, and *in the moment of their relaxation* coughing takes place, and mucus or pus is expectorated along with the air thus violently expelled; but when an opening is made into the trachea expectoration immediately becomes difficult, for the expiratory muscles have no longer their antagonist constrictors of the glottis to act against. They cannot therefore acquire sufficient force to produce that strong current of air which is at least highly favourable to expectoration. A patient, therefore, after bronchotomy, cannot perform the act of coughing, properly so called. He can only expire with a force proportioned to the quantity of air he can take into the lungs.

The truth of these observations is borne out by what I have noticed in all the cases in which I have seen bronchotomy performed. I have uniformly observed that expectoration immediately became difficult after the operation. The patient could not drive the expectorated matter through the canula, for the current of air was too weak; and hence I have seen death ensue from a small quantity of blood getting into the trachea.

It was my thorough conviction of this truth that made me close the external wound entirely, when I found the patient on the point of suffocation. The effect of the closure was, that the muscles of the glottis were called into play; the patient was enabled to cough, and consequently to expectorate, so as to get rid of the mucus which obstructed the bronchi; she, however, had a few hours before derived equal relief from a directly opposite practice, viz. the introduction of a canula into the wound; and the alarming symptoms had recurred only because the instrument had been displaced. This fact admits of a very satisfactory explanation. A full inspiration is favourable to expectoration, and the force of the expiratory muscles is in proportion to the quantity of air inhaled. The wound in the patient's throat, never very large, was considerably diminished by the swelling of the surrounding parts; she could not therefore take in a large quantity of air, while, on the other hand, she had lost the use of the constrictor muscles of the glottis; and therefore her expectoration was difficult or rather impossible. But when a large canula was introduced, she could take in a large volume of air, on which the expiratory muscles could act with such force as to produce expectoration. Both practices, therefore, though diametrically opposed to each other, were perfectly judicious under the circumstances of the case; but on far different grounds. The closure of the external orifice allowed the muscles of the glottis to act, and genuine coughing to take place; by the introduction of a wide canula, a large quantity of air found admission to the lungs, which is to a certain degree a substitute for proper coughing.

These considerations lead to some important practical views, and enable us to explain the fatal termination of many of the recorded cases of bronchotomy. In the *first* place, the canula employed ought always to be large. If it be small, the breathing will be difficult. During the inspiration blood will be sucked into the trachea from the edges of the wound, and a quantity of air sufficient for the purposes of respiration will not be admitted to the lungs. *Secondly,* When inflammation exists in the minute branches of the bronchi, attended with plentiful secretion, the introduction of a large quantity of air is absolutely necessary for expectoration. A small quantity of air cannot acquire sufficient force to bring up along with it the secretions of the bronchi. We can explain on the same principle why bronchotomy is so rarely successful when diseases have extended into the minute branches of the bronchi. After the operation the expectoration becomes more difficult, from the constrictor muscles of the glottis being rendered useless. Mucus accumulates more and more down the air-passages, and the patient dies gradually suffocated.

In severe cases of inflammation of the lungs the expectoration is commonly suppressed a short time previous to death, but it does not cease to be secreted; it is only not excreted; for, in proportion as more and more of the lungs becomes useless for the purposes of respiration, of course, a less quantity of air can be introduced at each inspiration, till at last the quantity is insufficient for a forcible inspiration or coughing, then of course the minute branches of the bronchi continue loaded, and the patient is suffocated. These observations on the influence of bronchotomy on coughing and expectoration were partially laid before the profession in the number of this Journal for July last; but the paper was too concise, and was misunderstood, particularly by the editor of the London Medico-Chirurgical Review. That gentleman may depend upon it that it is not wrong to say, that a patient with a tube in the trachea can most commonly only produce a current of air too weak for the purposes of expectoration. I have never seen a patient expectorate matter through the canula with any thing like freedom. It has been always necessary to extract the tenacious mucous by probes and other mechanical means. There has been uniformly a want of expiratory power which the constrictors of the glottis alone could have supplied. My professional friends have assured me that their experience has been of the same kind. They have witnessed the same difficulty of expectoration. The editor, likewise, as an argument against the truth of my views, objects, that, in the case formerly published, I had distinctly stated, that, on the first introduction of the tube, a fit of coughing supervened, attended to my great surprise with expectoration of semitransparent viscid mucus. I was certainly surprised, *because I had no reason to believe that any mucus was secreted by the bronchi;* but the expectoration of it immediately after the introduction of the tube is not only not in opposition, but very strictly in accordance with my views of the physiology of the windpipe. The patient had laboured under laryngitis, by which the size of the rima glottidis had been so far diminished, that a very small quantity of air could be introduced at each inspiration ; too little indeed for proper coughing. But as soon as an opening was made in the crico-thyroid membrane, and a large canula introduced, then the patient could breathe freely, and, though she could not cough, properly speaking, she could expire with considerable force, and thus get rid of a quantity of mucus in the way of expectoration.

From the foregoing statements I would draw the following practical conclusions :—1st, Bronchotomy should not be performed in cases where inflammation and copious secretion exist in the minute branches of the bronchi, at least such cases are most likely to terminate unfavourably. The operation, if performed, may relieve the breathing for a while, but it may occasion in a short time a suppression of the expectoration.

2d, We should always make use of a large canula for the sake of permitting free expiration, the only substitute for coughing, which the patient can no longer effect.

3d, *The operation of bronchotomy is not in itself unattended with danger ;* and even slight wounds of the trachea may be fatal from their effect in suppressing the excretion of the mucus of the air-passages. The case which I have just related is a good illustration of the manner in which a slight wound of the trachea may be fatal. The patient, a woman in the vigour of life, most carefully watched and attended to, died in consequence of a slight wound of the crico-thyroid membrane, because the inflammation extended down the windpipe into the bronchi, and was attended with secretion, which could not be expectorated on account of the wound below the glottis.

The same fatal termination had very nearly occurred in a young woman in the Royal Infirmary, a patient of my colleague Dr Ballingall, during the autumn of last year. She too had cut her throat slightly after her delivery of a still-born natural child. The wound was comparatively slight, and required no peculiar treatment ; but the inflammation had extended down into the minute branches of the bronchi, and was attended with copious secretion. While the wound was closing, the expectoration became so difficult that suffocation was repeatedly threatened, which was averted, however, by the enlargement of the wound, and the introduction of a large canula.

I remember to have surprised the house-surgeon, Dr Lubbock, by stating, that equal relief would have followed the complete closure of the wound by the thumb or fore-finger.

4th, The absence of expectoration may be either very favourable or unfavourable to the performance of bronchotomy. It is a very favourable symptom when it depends on the sound state of the mucous membrane of the bronchi. It is most unfavourable when it depends on such a closure of the rima glottidis that too small a quantity of air can be introduced to effect coughing.

Note.—Since writing the above, the author has been highly gratified by perusing in manuscript the case subjoined by Mr Craigie. He begs leave to assure that gentleman of his high admiration of his skill, resolution, and humanity, and to congratulate him on the successful termination of one of the most remarkable cases on record. He is also very much pleased to find his own observations confirmed by those of so judicious and acute an observer. W. C.

Art. IX.—*Case in which Tracheotomy was sucessfully performed.* By Mr William Craigie, Surgeon, Midclova, Aberdeenshire.

THE case in which the operation of bronchotomy was performed by Dr Cullen, as related in your ninety-second Number, has induced me to send you an account of a case, in which I performed the operation of tracheotomy; as it may suggest a plan for obviating the unpleasant and sometimes fatal consequences of that operation, from the patient's inability to *cough*, or throw out mucus or blood by the wound, or tube commonly employed.

Thomas Cordoner, aged 56, sanguineous temperament, was subject for many years to habitual cough and copious expectoration. On the evening of the 24th April 1825, passing near his house, I was requested to go and see him, as he was supposed to be dying. I found him sitting in bed with his arms extended, and grasping a post in each hand. Countenance inflated and purplish; eyelids dilated; breathing extremely difficult, with a sort of soft rattle as if in the trachea; voice almost suppressed, so that he was only able to exclaim in a whisper, "*Gie me breath.*" A considerable hard painful swelling, without perceptible fluctuation, occupied the left side of the neck, particularly under the angle and ramus of the lower jaw, preventing the jaw from being depressed, or the mouth opened to admit of an inspection of the throat; a similar but less considerable swelling also occupied the right side. He had been attacked three days before with pain in the throat and difficulty of deglutition, which, on the previous evening, began to produce difficulty of breathing, and on the morning of this day became very considerable, with suppression of the cough and expectoration. For several hours he had been expected every minute to die from suffocation. As swelling of the tonsils from acute inflammation was probably the cause of the breathing being impeded, *tracheotomy* was evidently indicated as the only means of saving his life.

The urgency of the case precluded the possibility of obtaining professional assistance or proper instruments, yet I determined to make an attempt to save if possible so valuable a life as that of the father of a young and dependent family. I accordingly prepared two quills, which became a substitute for a canula, and with my penknife proceeded to operate. The man being placed and supported on a chair with his body inclining forwards, I made an incision about an inch in length from the lower part of the thyroid cartilage downwards, in the mesial line, and then introduced the point of the knife under the cricoid cartilage, with an intention of dividing it and the cricothyroid membrane; but finding the cartilage resist the knife, probably from ossification, I turned the knife and divided the two upper rings of the trachea. One of the quills was instantly introduced, and the man on making a partial inspiration, and stimulated to cough, partly perhaps from the quantity of mucus in the trachea, but more I believe from the irritation of the tube, forced out a considerable quantity of mucus. The other tube being introduced he made a full inspiration, with an attempt also to cough, which filled both the tubes with mucus, but from its tenacity forced none out, it being retracted by the succeeding inspiration. During several following expirations only a small quantity of the mucus, now very slightly tinged with blood from the wound, could be got out by getting it entangled on a handkerchief, as it appeared at the end of the tubes. Fearing that the man might still be suffocated from the quantity of mucus, it occurred to me that if one of the tubes was stopped, the air and mucus would be more forcibly ejected through the other. I accordingly, during an expiration, closed with the point of my finger one of the tubes, and found the mucus thrown out in greater quantity, and with considerable force.

About an hour after the operation, when the breathing had become regular, free, and easy, and the patient had repeatedly expressed by signs his satisfaction and relief, the person who held the light fainted, and in his fall extinguished it. In the consequent confusion both tubes slipped from the wound, and before the light could be rekindled, and the tubes replaced, my patient was again nearly suffocated, the divided rings of the trachea having come together and nearly closed the opening.

About twelve hours after this operation the tubes again came out; but now I was rather surprised to find that the wound did not close as on the former occasion, the divided parts of the cartilaginous rings remaining separated nearly a quarter of an inch. I did not therefore consider it necessary to replace the tubes; and when the patient wished to expectorate, I enabled him to do so, by laying my finger across the upper part of the wound, diminishing the size of the opening, and enabling him to throw out the mucus. Soon after this I left the patient, directing one of the attendants to lay his finger across the upper part of the wound when expectoration was necessary.

At my next visit in the evening (of the 25th) the breathing continued free, and he complained of hunger and thirst, but could not swallow any thing. No apparent alteration on the swelling. On the 26th about noon, and nearly forty hours after the operation, a quantity of purulent matter escaped by the mouth and nose, and the patient was aware of swallowing a considerable quantity, and he was soon afterwards able to swallow some gruel with considerable pain, but with great relish. I saw him about 2 P. M., and on closing the wound with my finger, he could breathe in the natural way, and was overjoyed to hear again the sound of his own voice. It being unnecessary to keep the wound longer open, simple dressings were applied to it, and in eight or ten days it was completely healed. This man has since been as well as formerly. He says that he is better, his cough and expectoration being somewhat alleviated.

The fortunate issue of my case was no doubt highly satisfactory. Yet I know not whether I could be justified in performing the operation under the peculiarly unfavourable circumstances, viz. the want of proper assistants, of instruments, and of proper light; for we had only a sort of torch of thin splits of the resinous roots of the fir tree,—a common substitute for candle among the poorer classes in the country, and the extinguishing of this was attended with nearly fatal consequences. The quantity of mucus in the trachea seems also to have been an unfavourable circumstance, though I was not aware of this at the time. The quills were prepared with a slit at one end, to allow the two sides to be pressed together to enter readily, and keep the cut parts of the cartilage separate by its elasticity. A small notch was also made on each side to prevent it from slipping out too readily. My intention originally was to use only one quill, not as a canula, but for the purpose above stated, in regard to the cricoid cartilage; but I had fortunately prepared two, which formed a valuable substitute for the ordinary tube.

Midclova, Nov. 1, 1827.

ART. X.—*On the Poisonous Effects of the Mussel, (Mytilus edulis.)* By JAMES S. COMBE, M. D. Fellow of the Royal College of Surgeons, Edinburgh, Member of the Medico-Chirurgical Society, one of the Surgeons to the Dispensary, Leith.

THE exact nature of fish-poison,—the various circumstances connected with its production by, or adherence to the noxious animal—and the specific effects which it produces on the bodies of other animals, are still involved in great obscurity. This is partly owing, undoubtedly, to the inherent difficulties of the subject,—to our ignorance of the minute organization of the animals, their food, habitats, and diseases,—but principally, I conceive, to the ill-directed efforts of those to whom opportunities have been afforded of investigating this curious subject. This is certainly to be regretted, as few subjects can be considered more interesting both to the naturalist and physician. The inquiry has not been methodically or scientifically conducted, but left solely to those who have had occasional opportunities of witnessing its effects. Hence we have nearly as many theories as authors; for by those habits of hasty generalization, the besetting sin of our professional logic, each one has drawn conclusions which could only be true, if true at all, in his particular case, and not certainly in all the forms under which it presents itself.

I regret that it will not be in my power, in the following short and very imperfect paper, to throw much light on this subject. But in the present state of the inquiry it seems to be the duty of every one to contribute his mite to the stock of information, leaving to others more fortunately placed, or more highly endowed, to separate trivial from important, accidental from uniform and constant facts. I am likewise induced to lay it before the public, as it will afford most unequivocal facts to prove that a fish poison does occasionally exist, the occurrence of which no idiosyncrasy can explain, and which no candid inquirer can deny. Another reason prevails with me, that the cases may be considered unique, as there are none, so far as I know, in the writings of British authors detailing the appearances after death.

I was called on 21st June last to visit a poor man æt. 60, residing in Poplar Lane, whom I saw about 11 A. M. He was in bed, somewhat agitated in his manner; complained of considerable thirst and heat in his mouth; and had some difficulty in swallowing any liquid. He had also great desire to discharge his urine, which felt very hot. His pulse was small and weak; breathing natural; surface rather cool. His tongue on being pushed out with some effort was slighty turgid and highly coloured, without any fur. He complained of tension about the jaws and throat. He had a feeling of numbness about the lips, extending down his arms, the motions of which were irregular, not being entirely under his control. He had great difficulty in describing the sensations in his arms, which varied he said. His hands felt prickly. There was no eruption on his skin. He said that in the course of the morning, on attempting to rise to make water, he found that he could not stand; that in fact his legs were paralytic. I urged him to attempt it again; but although he could move them freely in bed in all directions, yet he dropt down utterly powerless on rising. Such a series of symptoms was sufficiently puzzling, and I was at a loss to form any opinion of his case after a minute inquiry. Luckily he said that he would not have troubled me to see him, as he thought he would be soon well; but that he had taken fright on account of a comrade of his, with whom he had supped, having died during the night. He asked me if I thought mussels could do him any harm; that both he and his comrade had taken a plentiful supper of them last night; and that he could blame nothing else. The case was now materially altered, and I was satisfied that the mussels were the cause of his illness. As they had been taken about sixteen hours, and had probably passed out of his stomach, I did not give him an emetic, but a smart cathartic, followed by some nitrous ether and alcohol. He said that he first got the mussels in the wet dock; that his wife had boiled them with salt to supper; that he saw nothing remarkable about them; that they had no particular taste; nor did he find any bad effects from them for several hours. I saw him again in a short time. The bowels had been freely moved, the stools presenting no peculiar appearance; his tongue was more swollen, and his speech was still more laboured. On making any exertion, as moving in bed, he was attacked with irregular contraction of the muscles, assuming very much the appearance of chorea. Towards evening he was so much restored, that, with the help of a chair, he could stand for a few moments, and his debility otherwise was not so great. The stimulant treatment was persevered in, and the bowels were freely moved.

On reaching home after my first visit to this man, I found a woman waiting for me to request me to see her husband. As it was not in my power at the moment, she told me his ailments; and they corresponded so entirely with those just related, that I had no hesitation in pronouncing them to be of a similar nature, notwithstanding the denial of the woman that her husband had eaten any mussels. I prescribed accordingly, and shortly afterwards saw him along with Mr Cheyne, to whom I mentioned the other case, and who was naturally desirous to visit this one. This man (Robertson) æt. 30, was a watchman in the docks; and in the course of the preceding evening, (20th) while on duty, had picked up a few mussels at the dock gates, which he saw several people gathering, and had eaten them raw. He is positive that the number did not exceed five or six, and that they were fresh, having no particular taste or appearance. The symptoms corresponded so exactly with those described in the other case, that it is almost needless to repeat them. He went to bed shortly after taking them, and experienced nothing unusual till next morning, except a slight burning sensation in the lips and tongue. He slept as he was wont to do. On attempting to rise in the morning he observed at once the powerless state of his limbs, and a slight degree of dizziness. At our visit, when he endeavoured to move and raise himself, he could so far command his muscles as to change the position of his body and limbs; but this he did in a jerking manner, and without being able to complete what he intended, suddenly losing the command of his muscular power. It appeared to Mr Cheyne that these irregular motions only lasted while he attempted to do more than his strength enabled him to perform. Under similar treatment he got speedily well, being able on the third day to resume his duty.

We now learned that many more cases had occurred, and medical men were seen in all directions hastening to afford relief in this sudden calamity. The town was in a ferment, and the magistrates with great propriety issued a warning against the use of the mussels. Many deaths were reported, and hundreds of individuals were stated to be suffering under it. Luckily matters were not so deplorable; but we ascertained, that, in addition to the man mentioned before, the companion of our patient, an elderly woman, had died. In all about thirty cases occurred, with great uniformity of symptoms, but varying very much in severity; but none, so far as I know, have left any permanent bad effects. They were entirely confined to the lowest ranks of life; and some of the hovels which we visited presented a sad spectacle of want and filth. The weak and aged suffered most, but no age from two to seventy was exempt from its effects.

We ascertained that the mussels had been taken in various forms and various quantities. By one or two they had been taken raw, and these persons seemed to escape from their fatal effects only by the smaller number which they ate. Most of them got them boiled with salt, pepper, and other condiments; and eight or ten of the sufferers had made a hearty meal of them. None, so far as I know, complained of any thing peculiar in the smell or taste of the animals, and none suffered immediately after taking them.

In general an hour or two elapsed, sometimes more, and then the bad effects consisted rather in uneasy feelings and debility than in any distress referable to the stomach. Some children suffered from eating only two or three; and it will be remembered that Robertson, a young and healthy man, only took five or six. In two or three hours they complained of a slight tension at the stomach. One or two had cardialgia, nausea, and vomiting; but these were not general or lasting symptoms. They then complained of the prickly feeling of their hands; heat and constriction of the mouth and fauces; difficulty of swallowing and speaking freely; numbness about the mouth, gradually extending to the arms, with great debility of the lower limbs. The degree of muscular debility varied a good deal, but was an invariable symptom. In some it merely prevented them from walking firmly, but in most of them it amounted to perfect inability to stand. While lying in bed they could move their limbs with tolerable freedom, but on being raised to the perpendicular posture, they felt their limbs sink under them. Some complained of a bad, coppery taste in the mouth; but in general this was an answer to what lawyers call a leading question. There was slight pain in the abdomen, increased on pressure, particularly in the region of the bladder, which suffered variously on its functions. In some the secretion of urine was suspended; in others it was free, but passed with pain and great effort. The action of the heart was feeble; the breathing unaffected; the face pale, expressive of much anxiety; the surface rather cold; and the mental faculties unimpaired. Unluckily the two fatal cases were not seen by any medical person; and we are therefore unable to state more minutely the train of symptoms. We ascertained that the woman (in whose house were five sufferers,) went away as in a gentle sleep; and that a few minutes before death she had spoken and swallowed.

With respect to the practice adopted we remark that it was uniform, and attended with evident good effects. In some the stomach was emptied by an emetic, and a few discharged some of the undigested mussels. In others brisk laxatives were administered, and followed by stimuli, such as spirits, ammonia, or nitrous æther. We have every reason to believe, that, had the two fatal cases been treated in time, the means would not have been less successful.

It became a matter of considerable interest to ascertain by dissection the exact nature of the effects induced by this peculiar poison; and we accordingly applied for leave in both cases. In that of the man it was only obtained by the warrant of the magistrates. But the examination was conducted under every disadvantage of a noisy crowd, and great hurry; we opened the woman's body in a sad hovel, her husband and children suffering severely in the same apartment.

The old man had taken a hearty supper of them, boiled, and some hours afterwards had gone to the glass-house, where he was night watchman, and was found dead in his box about three in the morning, *i. e.* six or seven hours after eating them.

We understood from a man who remained with him in his lodge during the night, that within a few hours after eating the mussels he complained of being unwell, and vomited. He lay down on a bench, from which he did not again move. Now and then he was seized with a universal trembling, accompanied by a fixed clinching of the hands. He spoke rationally till the last, and died as if by increasing weakness.

Externally the body presented nothing particularly worthy of observation. The abdomen was tumid and slightly tympanitic. In the course of the ileum there were several patches of suffusion of a dark red colour. The continuity of the coats was entire. The colon was constricted throughout a great part of its extent; rectum much inflated; stomach empty, presenting neither in its outer nor inner surface any decided morbid change. The bladder was considerably distended; and the orifice of the urethra was surrounded with a wreath of increased vascularity. No dissection of the brain or spinal chord could be obtained.

The woman who died had made a hearty meal of the mussels about twelve o'clock on 21st, and died about three hours afterwards. The body was examined nineteen hours after death. Great emaciation; abdomen slightly tympanitic; peritoneal covering healthy; one or two red patches in the small intestines; the stomach contained a large quantity of food, consisting of potatoes, mussels, &c. A minute examination could detect nothing decidedly morbid in any of the viscera. The bladder was full of urine.

These examinations were witnessed by Dr Anderson, Mr Cheyne, and others. They are, I must confess, exceedingly imperfect, and convey but little, and that only negative information. Yet we considered ourselves fortunate in being able to do so much; and it was done under every disadvantage.

It may admit of doubt whether a more minute examination would have detected any organic or unequivocal change in any part of the system. I am inclined to think not; but it will be the duty of every future observer more particularly to examine the condition of the brain and spinal chord, not omitting the fauces and pharynx.*

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* Since the above was drawn up, I am informed by the apothecary of the Dispensary, that in one of the patients the gastric symptoms were from the first very severe, attended with vomiting and spasm of the stomach and limbs. In a few days

It was a matter of immediate and urgent inquiry to ascertain the source of these noxious animals, in order not only to elicit all the information we could gather on so curious a subject, but to calm the agitation of the public mind. I may remark, that mussels abound on our coast, are used freely by the lower orders occasionally as a meal, and by the better classes they are served up with other fishes. Cases of the well known cutaneous and gastric irritation arising from their use are not uncommon; but I have not learned that any very serious or fatal effects have hitherto occurred in this place. In general, they are obtained from rocks on the coast; but of late they had been procured in large quantities from the walls of the docks, the waters of which had been discharged that they might be cleaned. We were assured that many poor people, indeed several of the patients had eaten these mussels with impunity; and the concurrent testimony of all concerned satisfied us, that the poisonous mussels were solely got from the bar at the dock gates. This bar, placed at the eastern gate of the western wet dock, was formed of two beams of fir, (Memel logs,) bound together by iron fastenings. It had floated there for fifteen or twenty years, and was drawn aside when vessels passed through the gates. It was hauled up on 20th June, and was found to be thickly incrusted with mussels, which being scraped off were thrown on the quay, and there found by the poor people who had suffered so severely. The wood was found to be perfectly sound, and being tarred was on the following day replaced in the water. We could not procure any of the noxious animals from this place, but a few were obtained from the house where the woman died. They varied in size as other mussels do; but could not be distinguished by any very positive mark from the healthy animals in the vicinity. My friend Dr Coldstream is of opinion that the liver was diseased, being darker in colour and increased in size. They were quite fresh; had no peculiar smell nor taste. The most delicate chemical tests gave no indications of cupreous impregnation; nor could a comparative chemical analysis of the poisonous and healthy fish, undertaken by Dr Christison, discover any peculiar principle in the former, or any difference in chemical nature between them.

I may mention, that the poisonous effects were not confined to our race, as a cat and dog were killed from having ate of the boiled ones in Poplar Lane. I have repeatedly tried the effects of those got in the immediate vicinity of the bar on dogs and cats, but without any sensible effect.

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this was followed by peritonitis, requiring the repeated use of the lancet. He ultimately did well.

It would be foreign to the object of this paper, which is intended solely to detail the circumstances attending the cases which fell under my own observation, to enter at any length into the general question of fish poison. The profession is already in possession of elaborate and judicious essays on the subject by Chisholm * and Burrows ; † the latter of whom, in an especial manner, has brought a great variety of information, medical, and scientific, to bear on this question, with that acuteness and judgment which might well unfold its mysteries if they were not so much beyond the usual sphere of our cognizance.

I have nothing to add to the information which these authors and some others have afforded us. But in so far as the legitimate consequences to be drawn from a consideration of our cases, may be allowed to influence our opinions on the question in general, it may be well to allude shortly to the more remarkable facts connected with it. If we cannot boast that the peculiar virus, or mode of action of the virus, has been detected in our imperfect investigations, yet I have no hesitation in affirming, that enough has been shown to raise doubts as to the correctness of the theories of some, and utterly to refute the hypotheses of others.

First, with respect to the symptomatology. It will be found, on reference to the authors both practical and systematic, who have written on this subject, that a considerable diversity of symptoms has been observed from eating poisonous fish, and even that species whose injurious effects we have described.‡ Indeed, so great is the difference, that I am inclined to suspect an essential difference in the nature of the exciting cause. Dr Beck enumerates the symptoms thus : " Violent oppression and agony; swelling of the face; a scarlet efflorescence over the body; insatiable thirst ; tormina and vomiting : and in fatal cases, coldness of extremities ; low and quick pulse, hiccup, delirium, and occasional coma." To these Orfila § adds, *menaces de suffocation*, subsultus tendinum, stertorous or convulsive respiration ; and should these symptoms not disappear under treatment, pain and other indications of inflamed mucous membrane supervene, which he seems to consider the most ordinary cause of the fatal termination. Foderé || corroborates this view of the inflammatory nature of the affection, by detecting after death a

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* Edinburgh Medical Journal, Vol. iv. p. 393.

† London Medical Repository, Vol. iii. p 443.

‡ Dict. des Sciences Medical. Art. Moule. Thomas, Mem. of Med. Soc. Vol. v. p. 94. Mochring, Haller, Disp. t. iii. p. 185. Beck's Med. Jurisprudence, p. 580, &c. &c.

§ Toxicologie, tom. ii. Part. ii. Leçons de Medicine Legale, tom. ii.

|| Traité de Medecine Legale, tom. i, v. p. 85.

phlogosed state of the stomach and bowels, in a case which proved fatal two days after eating mussels. Dr Thomas, who writes from actual observation in the West Indies, besides enumerating the ordinary gastric affections, says, that the face becomes flushed, and the eyes inflamed, attended with a burning heat and spasmodic twitches, which particularly affect the eyes; the sufferers often complaining that they are ready to start from their sockets." He mentions also a symptom which was found I believe, without exception, in all our cases, *i. e.* the irritation about the neck of the bladder, giving rise to strangury, retention of urine, and tenesmus.

The distinguishing features of our cases appear to be the very moderate primary, or secondary affection of the stomach, for in many there was no nausea nor vomiting, nor cardialgia. In none did the symptoms appear for a considerable, at least a well-marked period, after taking the mussels; and in none did the gastric symptoms continue prominent or alarming. It will be remembered, that several hours had elapsed since the patients had eaten them, without being sensible of any injurious effects till the nervous and muscular systems began to suffer. Another very characteristic feature, indeed the best marked and most uniform, consisted in the great debility, amounting in many to paralysis of the lower limbs, and of that particular sort which I have, I fear, very imperfectly attempted to describe. This is by no means a usual effect of fish poison; it is mentioned however by Dr Chisholm as observed after eating the conger eel, *(Murœna conger.)* Five individuals having partaken of this fish, in the course of the following night were all seized with violent griping and cholera, together with a peculiar sensation in the lower extremities, attended with violent convulsive twitchings, and particularly the child, faintings." The symptoms continued for a fortnight, and then terminated *in paralysis of the lower extremities.*

But the most striking coincidence of any with our cases, is to be found in the interesting account of the voyage round the world, by Captain Vancouver,* and occurred while he was examining the coast in latitude 52° 48′, N. (nearly our own,) and longitude 231° 42′. Some of the men stopped to breakfast, and finding some mussels, a few of them ate them roasted, as had been their usual practice. Soon after leaving the cove where they had breakfasted, several of the crew who had eaten of the mussels were seized with a *numbness about their faces and extremities;* their whole bodies were shortly affected in the same manner, attended with sickness and giddiness. The narrative goes on to say, that Carter, (one of the seamen,) attracted nearly the whole of their attention, but being unable to swallow the warm water, the poor fellow expired about half an hour after he was landed. "His death was so tranquil, that it was some little time before they could be perfectly certain of his dissolution. There was no doubt that this was occasioned by a poison contained in the mussels he had eaten about eight o'clock in the morning. At nine he first found himself unwell, and died at half past one. He pulled his oar until the boat landed, but *when he rose to go on shore, he fell down,* and never more got up but by the assistance of his companions."

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* Voyage of Discovery, Vol. iv. p. 45.

It is abundantly obvious, from the detail of the cases, that the virus possesses very energetic powers. The poor woman died three hours, and the man probably only survived about six hours after eating the mussels. And I am informed by a gentleman, that the debility and paralytic state of the limbs came on within an hour of eating two of them boiled, in a patient who was a delicate girl of six years of age. The effects would probably vary with the state of the stomach as to food and mucus, and the powers of the constitution to resist its deleterious agency.

I conceive the action of the virus to have been in all cases constitutional, *i. e.* that there was no immediate local action in the stomach, but that the virus being absorbed, it exerted its agency more particularly by depressing the nervous sensibility, and consequently the muscular energy.

Many various causes have been assigned to account for the deleterious effects of fishes. Many of them are sufficiently fanciful and absurd; but the following, although some of them are scarcely less questionable, are now most commonly entertained by medical men.

Cupreous Impregnation,
Putrefaction,
The Food of the Animals,
Structural diseases,—And
A *virus sui generis* pervading their whole system.

1. The first supposition has been so satisfactorily refuted by Mr Burrows, in reference to the poisonous fish of the West Indies, that it is only necessary to say, that the objections to its existence in our case apply much more strongly than in the former; for surely if the metal could have existed in solution so as to contaminate the mussels adhering to the logs at the dock gates, it would not less so to all those adhering to the walls of the dock; yet it is well known that the mussels got from the latter place were eaten with perfect impunity.

The idea is, however, I believe, almost entirely confined to the vulgar; and it is needless to add, that the most delicate chemical tests give no countenance to the supposition.

2. The putrescent state of the fish has been considered by some sufficient to explain the phenomena resulting from poisonous mussels. And it does appear probable that the extinction of life and the consequent putridity of the animal will allow any noxious principle to exert its agency with more certainty and effect; but further than this I am not prepared to go, and I do not think that it can be for a moment entertained as applicable to our case, although an anonymous correspondent of one of our journals, ignorant of the facts of the case, has revived the opinion, and assumed for it an originality worthy of a better cause.

To refute the opinion it is enough to state, that the animals were fresh when eaten,—that some of them were even living on the day subsequent to that on which the beam was placed on the quay,—that nothing particular was observed by any of the unfortunate people who partook of them, but that they were agreeable to the palate, and formed a savoury meal.

3. The next assigned cause, that of their food, may be dismissed as briefly. The food must have been of the same nature as that afforded to those in the neighbourhood, which were innocuous; and I am not aware that any injurious effects have been found to result from such food; and thus the same objection lies to this as to the former hypotheses, that supposing them to be founded in fact, they are unable to explain the effects that result.

4. A very competent judge, Dr Ferguson, has given his decided opinion, the result of observation and experiment, that the deleterious effects do not seem to be at all connected with any organic change in the animals; that so far from there being any marks of disease in the viscera, or other parts of poisonous fishes, they were found to be in the best season, and of the highest quality in all respects.* That some change happens in the structure or secretions of the animals appears very probable, but I suspect it is as much beyond the sphere of our detection, as its chemical nature is by the re-agents.

Lastly, I am, upon the whole, disposed to assent to the opinion of Mr Burrows, who conceives that the effects are best explained by attributing them to a poison *sui generis*; and it is better frankly to confess our ignorance of the peculiar circumstances which lead to its production, than to hazard assertions which the paucity of facts cannot substantiate. The great majority of cases occur in warm countries, or in the warmer seasons of more temperate regions; and our case does not upset this opinion. Yet even this is not without exceptions, for Fodcré, who makes mention of the old saying, almost amounting to a proverb, " Que les moules sont malsaines dans les mois ou la lettre R n'entre point," reports a case which fell under his own observation in France in the month of March.

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* Edinburgh Philosophical Journal, Vol. i.

The facts narrated in this paper, I humbly think, are sufficient to satisfy the most scrupulous of the existence of a peculiar poison in mussels,* contrary to the opinion of some authors, and to refute the idea entertained by others that their effects can result from any particular state of the stomach. To this latter opinion advanced by Dr Edwards, M. Orfila somewhat inclines. But it is not easy to imagine the existence at the same moment of a particular idiosyncrasy of the stomach occurring in so many individuals exposed to the influence of the noxious matter, and producing in all the like effects. M. Orfila places mussels in the class of septic poisons, and his definition of that class answers very well to the general character of our cases. " Poisons septiques sont ceux qui determinent une faiblesse generale, la dissolution des humeurs et des syncopes, et qui n'alterent point en general les facultés intellectuelles."†

ART. XI.—*On the Coup de Soleil.* By JAMES MITCHELL, Esq. Surgeon, Royal Navy. Communicated by Dr Duncan Jun.

THIS disease has been termed by the French " *Coup de Soleil*," or stroke of the sun, thereby expressing the chief cause of its formation. Cullen seems to consider it as a symptomatic species of phrenitis, and terms it " Phrenitis Calentura" in his System of Nosology, although I do not observe that he takes notice of it in his work on the Practice of Physic, except that he enumerates among the causes of phrenitis, exposure of the naked head to the sun's rays. Dr Thomas, in his Practice of Physic, speaks of this disease as a species of apoplexy. Certainly, in some cases, the stroke of the sun produces apoplexy, or something very much resembling it; and when the system does recover from the stroke, it is not to be distinguished from apoplexy, either when life remains, or afterwards by dissection, the appearances being the same. But, in general, it seems after the stroke to assume the character of phrenitis; and, in my opinion, Dr Cullen more properly classes the disease as a species of phrenitis in class *Pyrexiæ*, order *Phlegmasiæ*. I therefore think the name of *Ictus Solis*, or *Coup de Soleil* of the French is the most expressive term for it, as it comprises its apoplectic as well as its phrenitic character.

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* Stone on the Stomach.

† Toxicologie, Tom. ii. p. 449.

The effects of the stroke of the sun were not unknown to the ancients in warm climates; hence we find in the Sacred Writings allusions to the effects of the sun in producing severe disease, and sudden death.

I may adduce the following passages respecting the knowledge of the sacred writers respecting the influence of the sun on the human frame. Psalms, cxxi. v. 6, " The sun shall not smite thee by day, nor the moon * by night." Isaiah, chap. xlix. v. 16, " They shall not hunger nor thirst, neither shall the heat nor sun smite them, for he that hath mercy on them shall lead them, even by the springs of water shall he guide them." Jonah, chap. iv. v. 8, " And it came to pass when the sun did arise, that God prepared a vehement east-wind; and the *sun beat upon the head* of Jonah, that he fainted, and wished in himself to die, and said it is better for me to die than to live."

Various other passages might be adduced to prove the effects of the sun on the human frame, and the blessings of shade in foreign countries. But these may suffice.

Coup de Soleil or sun-stroke is known to occur in all hot climates, particularly in their hot seasons. When surgeon of the naval establishment of Canada, Lake Champlain, in the summer of 1815, I had a great number of cases, with one exception all marines, and all of which occurred in the course of two very hot sunny days. They were attacked with it at their posts with their muskets in their hands; and such was the fury with which they were seized that they actually made a charge. I have observed three kinds of this disease:—1st, Sudden and complete insensibility, attended with total loss of power, stupor, and stertorous breathing.—This is the apoplectic species. 2d; The immediate appearance of symptoms of violent phrenitis with delirium. The 3d, consisting of symptoms of chronic phrenitis, coming on gradually, and continuing a long time, attended by delirium of a particular nature ; sometimes violent when the patient was treated kindly, but in general being attended with a perversion of ideas.

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* David, in the Psalm above alluded to, mentions the stroke of the moon as well as of the sun. Now, this is an allusion to a most singular effect of the moon, although it cannot be from the heat it emits. However, as David says, it does produce a stroke of some kind. I have heard of its producing paralysis of the faces of sailors when exposed sleeping on deck in the full moon, their mouths being drawn awry, and never regaining their usual appearance. Once, when at Java, a sailor was under my care for intermittent fever, apparently produced from sleeping on deck in the moon, attended with very anomalous symptoms, such as sudden swelling of the face, delirium, livid spots on the face and countenance, appearing and disappearing at irregular intervals, and a paralytic affection of the mouth and tongue. The people and himself attributed it all (on my inquiries) to his sleeping in the moon; but I judged it rather to arise from his exposure to the concentrated marsh miasma which abounds in the night-time, and is so fatal to Europeans in that climate, though I would not go the length with most of my brother surgeons in his Majesty's service, who have treated the notion of the moon as ridiculous, and merely to have originated in the superstitions of the sailors. The moon has certainly some influence as well as the sun in producing or modifying diseases. It is very clear, that fish or meat exposed in warm climates to the rays of the moon, not only turns putrid, but are in some cases rendered poisonous.

The 1st species, which I have termed the apoplectic, is a very fatal disease. It generally occurs in persons of full habit, living freely, and suffering much from heat ; in consequence of being ordered on some arduous service (when the stomach is distended with food, and the blood-vessels excited by ardent spririts,) under a burning sun ; or after exposing the body to the sun in the act of sleeping after a drunken fit, as I have seen sailors do.

In these cases the pupils are remarkably dilated, the patient insensible to external stimuli ; the features of the countenance distorted and distended, seemingly as if there was engorgement of all the vessels ; the pulse slow and labouring, sometimes intermitting ; the veins of the neck and head of the subcutaneous order seem frightfully engorged ; and, indeed, such is the determination to the head, that the skin of these parts seems as if it had undergone a most successful injection, or was in a high state of inflammation. Sometimes paralysis of the sphincters takes place ; all sensibility is gone ; and the patient lies in whatever posture he is placed. The extremities are generally cold ; and if something is not instantaneously done death soon closes the scene.

A sailor when on shore had drank a great deal of ardent spirits. After this, on leaving the canteen, he went to a retired place in the open air, lay down to sleep, exposed to the rays of a vertical sun, and he was found out by accident shortly afterwards in an insensible state, not from the influence of spirits, but from the effects of the sun on him in that state of debility from sleep and spirits. The messmates thought him drunk, and had him put to bed as such. On my arrival, about twelve hours afterwards, he was reported to me as in that state ; but having some suspicion about the case, I examined him, and found him to be labouring under the above-mentioned disease. He was taken to the hospital, where, by the exertion of very powerful means, I gradually restored him, though it was a month before he was fit for duty.

I confess it is very difficult to distinguish in some cases of this nature, whether apoplexy has taken place, or whether the patient is merely labouring under the operation of intoxicating liquors. It is requisite to understand the diagnosis accurately, to determine the proper mode of treatment, as what is absolutely necessary in the one case might prove fatal in the other. Since writing the above, various cases of this species of the stroke of the sun have occurred in England this summer, in consequence of the great heat, generally attacking labouring people in the fields engaged in agriculture, and almost in every case proving quickly fatal. In one newspaper I observed no fewer than twelve cases of sudden death from this disease.

In this state the vessels of the brain become turgid; and, by being once distended, they cease to circulate the blood, and thereby produce alarming compression of the brain, generally becoming speedily fatal by the rupture of a considerable blood-vessel, or more slowly, by a small blood-vessel giving way, or by effusion of serum; and before the system rallies sufficiently to absorb the effused fluid, a fatal termination ensues.

In this species of the disease every thing depends on the most speedy and energetic treatment; for if something is not done instantly when the patient is seized, it is very unlikely that any future treatment will be of use. The treatment I have found to be most efficacious is the following: 1st, A very large general bleeding, so as to produce complete relaxation of the system, either from a vein of the arm, from the temporal artery, or what is much more efficacious, viz. from the external jugular vein, which seemed to me to act more decidedly in withdrawing the blood from the vessels of the head, and thereby relieving them from that dreadful engorgement which proves fatal either by a rupture of some of them, or from effusion being produced. 2d, The head to be shaved, and cold fluids to be poured on its centre from a height. 3d, The lower extremities to be placed in very hot water. 4th, Stimulating purgative enemas to be thrown up the *rectum.* 5th, The whole body to be rubbed with stimulating embrocations. If these means are so far successful in preventing a fatal termination, the necessary after-treatment is the same as will be required for the other species to be taken notice of afterwards. Could a medical man be found immediately, and these means used, I think this species of the disease would not prove so rapidly and generally fatal as it does. On one occasion, when having landed on an island in Jarvis's Straits, which was hardly any thing but a naked rock, and that nearly white from the dung of sea-birds, I suffered much from the reflection of a burning sun, and indeed thought I was on the point of having a stroke of the sun, as I nearly lost my vision, and my head felt distended as if ready to burst. I rushed towards a cave in the rock facing the sea, and finding salt water in it, I took a handkerchief, and with it kept my head in a constant state of evaporation. In fifteen minutes I felt myself completely relieved from the most unpleasant sensations. When my companions joined me, they were all suffering much from the intense heat, though by no means in the proportion that I did, as their habits were not equally plethoric. They followed the plan that I had adopted, and soon experienced relief.

The second species, which I term Phrenitic Acute, bears a very strong resemblance to phrenitis in its symptoms, except that it occurs in an instant without previous warning. I think that it cannot be better described both in its history, symptoms, causes, and treatment, than as it appeared to me on service in Canada, during a very hot summer, about ten years ago.

I was stationed as surgeon to a naval establishment on an island in Canada, near one of its celebrated lakes. The island was quite free from trees or shelter. The sun beat freely on every part of it, and was very oppressive in summer; but in the particular season alluded to, the heat of the sun was so excessive, that in Montreal horses dropt down dead in the streets, and the bee-hives discharged their precious contents. Our marines on sentry were ordered to keep their boxes for shelter, having little windows of observation cut in them. But such is the carelessness of these men, with regard to the preservation of their persons, that several of them scorned the advice, and walked about, and thereby to the number of seventeen or thereabouts, suffered this species of the stroke of the sun. They in an instant became quite deranged, charged with their muskets, pushing at every thing they met with in their route, till they were seized by force, and conveyed to the hospital. On inspecting them, I found great muscular strength exhibited, continual struggling to get free from the necessary restraint put on them; the muscles and all the blood-vessels much distended; eyes red, protuberant, and staring, pupil contracted; face swelled, red, and all the features distorted; the neck turgid and red; jugular veins much enlarged, the carotids pulsating with great violence, so as to be seen elevating the surrounding integuments. All these cases occurred in the space of a day or two.

As I dreaded from the violence of the symptoms effusion of blood or serum, I immediately determined on a large general bleeding from the arm, *usque ad deliquium animi.* From the first case, which was a very violent one, as the man was of a very plethoric habit, it was requisite to take away about five pounds of blood by measure before I could induce fainting, and consequent cessation of the violent vascular and muscular action. The relaxation of the system was then so great, the sphincters becoming powerless, and syncope was so complete, that I thought I had carried my plan too far. The system soon rallied, however; and, in four or five hours more, the symptoms returned though not with their former violence, and I was under the necessity of abstracting two pounds more. In none of the cases did I abstract less than three pounds the first time, and less than five pounds at the two general bleedings, not being under the necessity of repeating them more than twice, as they had the most happy effect. The only other remedies necessary were shaving the head and keeping the surface in a continual state of evaporation by cloths wet with vinegar and water; exhibiting purgatives and enemas, with cooling acidulous drinks. They all recovered, though the cure was tedious, owing to the great debility ensuing after such increased action, and consequent extreme depletion.

After this several persons in the vicinity were attacked with the same disease. At first they were treated by small and frequent topical bleedings; but two died under the treatment; and on dissection effusion of blood was found in one case,* and of serum in another, which was more protracted; therefore, larger bleedings were had recourse to, and this proved successful. The bleedings were not to the extent of mine, as they were not able to bear so much loss of blood as the sailor, from the latter being more plethoric owing to his confined life, and having a more full diet, and more spirituous liquors. I do not mean to say that I would take so much blood perhaps in this country were similar cases to occur, (and I am persuaded they often take place in hot summers;) but my great object would be if possible on all occasions to produce a complete state of syncope. I met with two cases of this disease at Rio de Janeiro about four years ago, and I carried the depleting system to as great an extent, and with the happiest success, though they were both men of spare, and by no means of plethoric habits.

The third species I call Phrenitic Chronic, because it comes on rather gradually and insidiously, and may continue a long time without destroying the powers of life. It seems from the cases I have seen to have during its continuance a tendency to disorder the arrangement and association of ideas rather than to destroy vitality; and though I have not been able in my limited knowledge of this disease to find out that it ever terminated in permanent mania, yet I have no doubt it contributes its proportion to the formation of that disease, particularly when persons are obliged to remain in a hot climate, where it is chiefly engendered. In all the cases I have known the patients were ordered to England to enjoy the bracing air of its climate; and their ideas never became correct till they got out of the hot into cooler latitudes, when they awoke as it were out of a dream, and wondered how they had remained in such a delusion and train of reasoning and ideas so foreign to them.

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* Chisholm in his Manual mentions that in fatal cases of sun-stroke in the West Indies, it was not uncommon to find blood effused in the ventricles of the brain.—Ed.

As far as my experience goes, I have observed this disease only in hot climates. The patients were persons in a superior sphere of life, who had been well educated, men of very acute feelings, particularly in all points of their duty as officers; for I have known none of the lower orders afflicted with the malady.* The disease began by the person suffering a dull headach for some days, and considerable feeling of distension of the head, as if the brain was confined by the skull, and unable to expand itself. The countenance was turgid, sometimes flushed and pale alternately; many vessels were seen on the conjunctiva carrying red blood; pulse quick and noisy; tongue much loaded in the centre, with a red edge; rather dry in that part. The patient's mind is at the same time much agitated, particularly about his official business; every trifling joke is converted into a serious insult; and he is very irascible, particularly towards those who are his best friends. He seems to be continually engaged in a bustle; his mind is so incessantly occupied that he cannot sleep, nor will he take time to eat his food, if placed before him. He swallows it as it were unconsciously, without chewing it, talking all the time, loquacity being a prominent feature of the complaint, the language good and the reasoning excellent. He engrosses the whole of the conversation, and directs it to himself, his situation, and affairs; but he speaks loud and dictatorially, without his usual prudence, and seeming in no wise to care whether he offends or not, considering his situation desperate. Every contradiction throws him into a perfect paroxysm of passion and violence. At this time, on particular observation, there will be found a tumefaction of the right side in the hypochondriac region, though without pain on pressure. This seems to arise from engorgement of the liver from bile, as little or none passes with the stools; and indeed, as may be supposed, great costiveness is prevalent; the stools are hard, and of a light clay-colour; and the urine is very scanty, and much loaded. There is no jaundiced appearance prevalent. This alarming state of the frame is not observed by the patient himself. He thinks his health is unimpaired; that he is merely oppressed with multifarious duties; and that as soon as they are finished every thing will go on as usual. In this state he may proceed perhaps for weeks, sleeping and eating little or almost none, merely drinking a great quantity of fluid on account of the great and incessant thirst existing from the intense febrile action that prevails. At this time the skin is quite arid, and perspiration not visible, though the heat of the weather is great. The strength of the body is not diminished, but, on the contrary, much increased, and the patient goes about, and is able to make very great exertions.

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* Officers both military and medical, are most frequently affected with this species, when, in the discharge of their duty, they are exposed to the sun's rays, where the heat is not so excessive as to produce the former species. We have seen examples from the East Indies and Sicily.—Ed.

Previous to these symptoms the patient had been harassed by various arduous duties and heavy responsibilities, and required to make great exertions, when exposed to the rays of a tropical sun, sometimes in boats, at other times on horseback, and often marching to great distances, with feelings much goaded, perhaps on account of his not being able to accomplish the service upon which he was sent, in spite of every exertion of his own, to the risk even of losing health or life, while others who should have assisted him were unable or unwilling to afford the expected co-operation. Under these circumstances, with the mind anxious and the body exhausted, it cannot be wondered at that a tropical sun should have such effects on those who had previously stood its powerful rays with impunity.

If at this stage of the complaint the patient is not attended by a skilful physician the event must be lamentable indeed; things will go on from bad to worse. The practitioner should proceed in the case with the greatest gentleness and caution. It will require all the address of the most consummate politician to get the patient to receive visits and to take medicines, as it is difficult to convince him that there is a necessity for either the one or the other. He requires to be courted and won by the most soft and soothing language of persuasion and friendship. His physician must reason with him calmly on the heat of the weather, impress the consequent necessity of keeping within doors, cool and quiet, and the propriety of removing the bile by purgatives, a caution which is known to be necessary by all persons, not medical men only, who frequent hot climates. His diet should be very mild, cooling, and laxative. All stimuli, either in the way of food, clothing, or mental excitement, should be most studiously avoided. By this plan in the course of a short time it may be completely removed.

But, on the contrary, should a different plan be pursued, such as the use of restraint or violence, and plainly pointing out to the patient that you consider him insane, the disease is greatly aggravated. The eyes become red and ferrety; the countenance more flushed; and there are evident symptoms of determination to the head. He then grows jealous of every one, and suspects and dislikes his best friends, as he is well aware, that, though their intentions are good, they are taking means to secure him, and he is therefore continually on his guard respecting all their advices and proposals towards him. Should they succeed in securing him he becomes perfectly frantic; and the most violent measures in the way of depletion are then had recourse to, in order to remove the disease. Improvement, however, does not take place until all restraint is removed, and perfect confidence is restored between the patient and physician.

I do not mean to say that restraint in this species always brings on the maniacal attack, or that it would have not occurred spontaneously. But in one case that came under my own observation, it was decidedly brought on by ill-timed and ill-judged measures. When this state is induced, the delirium is chiefly monomaniacal. The person is perfectly aware of the presence of his friends and acquaintances when they visit him; able to answer readily, and with very great shrewdness, any question which is put. The answer is rational, unless when they touch on subjects connected with his particular hallucination. He reasons very logically, and with great acumen on all subjects introduced, though there be great volubility of tongue, anxiety of the countenance, and extraordinary opinions are advanced, with a large proportion of the ludicrous in all his remarks. During the whole of this state the bowels are so much constipated, that ordinary purgatives have little or no effect. The secreting function of the liver seems to be decidedly deranged, indicated by evident swelling in the hypochondriac region, and also by the stools being perfectly white and destitute of bile. The appetite is entirely gone; the thirst is trifling; the skin quite dry and dingy, of an anserine appearance; and indeed the body throughout puts on rather a jaundiced hue.

Even under the very best treatment the delirious train of ideas does not subside for a very long time; and the patient betrays such a degree of eccentricity, both in words and actions, that it appears doubtful whether he will ever be restored to his former state of sound feelings. In none of the instances I saw were ever the delirious feelings removed, till the individuals had left the warm climate and got into a colder one; when they looked back with amazement and alarm, on their former ideas on subjects in which they were now perfectly lucid. The disease, indeed, and the remedies generally used for its removal, completely destroy the tone of the nervous system

Should the disease from neglect or bad treatment terminate in maniacal delirium, the usual plan that is had recourse to is large bleedings, both general and local; severe purging; blistering;

cold baths, and almost total abstinence from food. At the same time, violent coercion is employed. The result of all this is in general a complete loss of tone in the nervous system, death, or confirmed insanity. As such are the dismal effects of this treatment, it is necessary some other plan should be adopted. I consider this disease to be chronic phrenitis, produced by a relaxation, and consequent fulness of the vessels of the membranes of the brain ; but particularly of the delicate arachnoid coat, arising from loss of equilibrium between the circulating and nervous systems, evidently the product of debility. Whatever tends to recover the tone of these vessels and the balance lost between the circulating and nervous power must remove the disease, unless there be some organic change existing in these parts. These inferences are justified by the appearances on dissection of persons who died while labouring under this disease.

In this stage of the disease there is neither headach nor intolerance of light to indicate acute inflammation of the cerebral membranes. On the contrary, there seems to be a temporary paralytic state of the nerves of these parts, and hence an inability to propel the blood, producing stagnation, and consequent distension of the vessels. I cannot agree with Pinel and Esquirol, who suppose in this state of disease that the digestive organs are the first in fault. In the cases which I have observed, and I have done it very narrowly, the brain and nervous system were the first affected, and through them the circulating and digestive organs became deranged. The system of indiscriminate bleeding in all head cases, and to a great extent, has done an immensity of harm. Serous effusion has been induced, and the event which the medical man dreaded was hastened by the step he took. It certainly requires attentive examination into the history of the patient, into his previous habits and former complaints, to ascertain when bleeding is necessary in these cases. The young and inexperienced practitioner, when he gets a case of inflammation of the brain, or other viscus under his care, thinks he can never do wrong if he bleeds till pain ceases, and all symptoms of inflammation are gone. But how many has this practice sent to their graves ; and hence the great necessity of careful observation, to trace the line of demarcation between the acute and chronic stage of the inflammation. The moment that the chronic state is observed a very different plan of proceeding is necessary ; and though the necessary quantity of blood should be drawn, the patient should lose no more ; for it is astonishing what injury is done by any undue quantity of blood taken away, after the acute state is removed ; viz. in phrenitis, producing effusion between the membranes, and in the ventricles of the brain; in carditis, causing hydrops pericardii; in pneumonia, inducing hydrothorax or phthisis; in gastritis, leaving great sinking of the vital powers; in peritonitis, causing ascites; in enteritis tympanites; and in rheumatic fever, a complete break down of the constitution. All this appears to me to occur from the balance being destroyed between the circulating and nervous systems, and by the waste of that electric nervous fluid, which gives tone to the animal fibre.

The treatment I would recommend for this state of the disease, when the maniacal state has come on, would be gentle purging with aloes and canella, assisted by enemas, draughts composed of the acetite of morphia, camphor, and antimony, in large doses, aided by the warm salt-water shower-bath, (taking care to rub the body well with coarse cloths, and then place it in blankets,) so as to allay nervous irritation, induce sleep, and promote determination to the surface. General bleeding should be most sedulously avoided; and if depletion be required at all, it should be done by the aid of leeches to the temples. The head should be shaved, and kept cool by cloths wetted with vinegar and water. A blister may be applied between the shoulders, or, what is much better, a seton may be placed in the nape of the neck. If it appear that the disease is complicated with an hepatic affection, indicated by a puffy swelling in that region, with pain on pressure, and the other usual symptoms, then a cluster of leeches should be applied to the pained part, followed up by a large poultice, giving internally some brisk purgatives of calomel and jalap. Nourishing food, with malt liquor, should be given, and great attention paid to cleanliness.

The nurse who attends must be a person of firmness, to prevent the patient doing any injury to himself or others. At the same time he must have a good temper to bear the whims of the patient, and to treat him with the greatest kindness and humanity, soothing language acting like an opiate or charm; whereas contradiction, harshness, and violence, only aggravate the disease by exciting the maniacal paroxysm.

As this maniacal state bears the character more of a moral than a medical disease, I think great and good effects may be gained by permitting the visits of sensible religious friends or clergymen, with soothing language and persuasive arguments, to converse with and reason upon the absurd religious notions the patient has adopted, in consequence of the diseased state of his imagination and perversion of his judgment, such as considering himself the Saviour of the World, or some great prophet, or the harbinger of the millenium. This latter idea, singular to say, prevails most in mania, the product of a "*coup de soleil.*" They should not talk to the patient as if his opinions were absurd; but state how others were affected similarly, and the patient will perhaps apply this reasoning to himself. The clergymen or friends who are allowed to visit should be persons of considerable abilities and ready-witted; for I have observed that when the disease has not ended in fatuity or effusion, the patient has great brilliancy of ideas, and his reasoning powers are very acute. Were he therefore to get the mastery in argument, it would only be confirming him in his absurd chimeras; but, on the contrary, were he good-humouredly foiled in his notions, he would discard them, and thereby recover his reason. On the patient's recovery he should live in close retirement for many months in the country, avoiding all business and society that would produce any mental exertion, so that the brain and nervous system may regain their tone, which is best done by the absence of all mental stimuli. Great care should be taken to keep the bowels open by gentle laxatives and moderate exercise.

If this plan cannot be adopted at home, either from the obstinacy of the patient, or inability to procure a proper attendant, in that case he should be placed in a public or private asylum for lunatics. These places were formerly dreaded justly both by patients and their friends, but are now in many instances carried on in a highly creditable manner. From the enlightened state of the country, and from the investigations of Parliament, a new light has broke in upon these establishments. It is now known that it is not necessary to treat the maniac as a criminal, or that he should be flagellated or starved by cold and hunger. The time has now arrived when the patient may be deprived of his liberty, yet enjoy the freedom of his limbs, and thereby benefit his health. It is also found, that though he may be deprived of his reason, yet the feelings of hunger, thirst, and cold, and heat, are still in existence, and must be attended to by an adequate supply of wholesome nutritious food, fluids, clothing, fire, and exercise.

I could adduce various cases in proof of the justice of my remarks on this disease, (*Phrenitis Chronica,*) produced by the great heat of the sun; but one may suffice.

This occurred to an individual in the army of the Duke of Wellington, after the action of Talavera in Spain, while on the masterly retreat of his army into Portugal, in order to occupy the lines of Torres Vedras. The subject of it had to perform some arduous duties, during which he was much fatigued and long exposed to an ardent sun. After this he became restless night and day, agitated by trifles. On any thing pleasant occurring, he was overpowered by joyous feelings, that rendered him very loquacious. His whole manner gave him the appearance of having drank too much. He felt no inclination for eating, and when he did eat, he swallowed the morsel without masticating it. He was always thirsty. Excessively melancholy on the most trivial occasions, when in his excited state he became very irritable on the most trifling occurrence. Sleep he could only procure on alternate nights, and even then very little of it. This state continued for some time, during which he went about doing duty. All the symptoms quickly became aggravated. His head felt as if it were distended, or as if the brain was becoming too large for the skull; and his eyes also felt full, and appeared very dazzling. His pulse was quick and irritable, and his tongue foul. Shortly after, he felt pain in the region of the spleen, then in the right lobe of the liver. The stomach then became affected by pain, vomiting, and severe heart-burn. After eating or speaking much, or becoming excited by any means, mouthfuls of food hardly changed were ejected from the stomach; and indeed complete and alarming dyspepsia came on. The bowels were sometimes costive, but more frequently lax. The feces were of a colour that indicated a great deficiency in the biliary secretion.

In this state he applied to the surgeon of the regiment, and hoped that he would relieve the state of his head by ample bleeding; but this the surgeon absolutely refused. After inquiring fully into the state of the case, and finding, from the length of time that he had been afflicted, that it was of a chronic nature, he ordered his head to be shaved, and to be kept covered by cloths continually soaked with vinegar and water. He opened the bowels well by calomel and jalap, assisted by draughts of infusion of senna with Epsom salts. The patient became more and more confused, till reason entirely left its seat, and a maniacal paroxysm was induced. A complete cap-blister was put on all over the head. Soon after this he recovered his reason; nourishing diet was given him; and by the aid of a few laxatives he was able to return to duty in a fortnight.

About four months afterwards, in 1811, similar causes produced a very slight attack. He felt it coming on, and applied to the surgeon; but confinement for a few days, and gentle opening medicine, removed it. He had no attack until the year 1814 when in Portugal, on the point of embarking for England. His mind was greatly distressed on hearing of the death of a near relation, and a slight relapse was the consequence; but confinement to his room for a few days, and mild treatment, removed it. After this he travelled much, crossed the Alps, and passed through Italy. He was at the action of Waterloo; and was in France afterwards with his regiment. He felt no return of his former malady until the year 1819, when it was quickly removed by gentle laxatives, and he was not under the necessity of keeping within doors. He remained in good health in Scotland until July 1826; a summer that will be long remembered in Great Britain for the intensity of its heat, great drought, and premature harvest, as also for the number of cases of bilious fevers, cholera, diarrhœa, and *coup de soleil*, chiefly of the species I have termed apoplectic, terminating fatally. At this period he exposed himself without his hat for many hours working very laboriously, until the perspiration was pouring from him, which he was never accustomed to do. On going to tea in the evening he had no appetite; and he felt his head confused. At last intense headach came on, attended with stupor, and followed by complete delirium. An able and highly esteemed surgeon, who was called, bled him largely, in all at different times to the amount of 100 ounces, from the arm; shaved the head; applied a large cap-blister to it; opened the bowels freely; and enforced a most rigid antiphlogistic regimen.

After this the disease assumed the chronic type. He was sane on most points, but evidently insane on others. He had lost all recollection of dates and events, and forgot everything that was told him only the minute before. The surgeon never inquired particularly into his case, or whether he had been similarly affected formerly; he therefore continued reducing his patient by giving him daily purgatives for nearly two months; and considering the liver to be affected, from his patient having been in a warm climate, he earnestly urged an alterative course of mercury, to which his patient objected, and rebelled against taking any more medicines. At this time his weakness was so great, that he passed his urine and feces involuntarily. He complained of faintness from hunger, but was allowed little or no solid food. A consultation of medical men met on his case, and advised him to be sent to a private asylum for the insane. On his admission he was immediately put on a mixed diet of animal and vegetable food with malt liquor. I saw him a few days afterwards, and was delighted to find him walking about, but still insane. A seton was put in the neck, and in eight or ten days he became quite sensible, and increased in strength daily; and the account that I had from him afterwards was, that he found great difficulty in restraining his appetite, which was ravenous. The history of this case demonstrates the advantage of treating maniacal or head cases in the mildest manner, with little or no restraint, and by nourishing diet. It presents some difference from my general statement respecting the loss of recollection; but, as it elucidates best in other respects what I wished to prove, I think it worthy of being quoted.

Art. XII.—*Memoir of the late Dr* Robert Jackson, *Inspector of Army Hospitals.* Read by Dr Borland at the Anniversary Meeting of the Officers of the Medical Department, 12th May 1827.

Gentlemen,

In this appropriate place of our annual meeting, surrounded by several senior officers of the department, to whom the inestimable qualities of a distinguished member, now no more, the venerable Dr Robert Jackson, were well known, I proceed, at the request of the Director-General, to perform the duty assigned to me, of offering the tribute, justly due from us all, to the memory of a man whose useful labours must always be highly appreciated, not only by this department of which he was an ornament, but by the members of the medical profession in every quarter of the globe. *

The Island of Jamaica was the field in which Dr Jackson commenced his professional career. The medical charge of a detachment of the 1st battalion of the 60th regiment, quartered at Savanna-la-mar, in the year 1774, was the first duty on which he was employed with the army; and his records of the cases of disease which occurred during his medical superintendence of this detachment, which comprised a period of several years, have been preserved, and exhibit specimens of accuracy in clinical description never surpassed. They are the first fruits of a rule (carefully to register every occurring symptom,) which he then laid down for his guidance; to the undeviating observance of which afterwards, in every condition of service,—in camp or in quarters,—ashore or afloat—may be ascribed the high reputation he attained. As this rule offers an instructive lesson and example, the junior officers of the department who are now present, will, I am sure, readily permit me to draw their attention to the circumstances which led him to adopt it. When Dr Jackson entered upon his duties with the 60th regiment at Savanna-la-mar he tells us he had already seen something of common practice, under common practitioners; but the results did not correspond with his expectations. Desirous to know what is truth, and disposed not to be satisfied with less than truth, he proceeded to seek for it in his own experience. He had studied anatomy, and considered the laws of the animal economy with care and attention. With this preliminary information he believed that the knowledge of the correct history of disease, and of the most effectual manner of removing it, would be best attained by writing down at the bedside of the sick, military or others, a summary of the rise, progress, and issue of the maladies which came under his notice, by marking with care the changes which took place,—the medical means prescribed,—the effect produced by the prescription, so far as it was discernible, and by adding, as often as leave was obtained to open the dead body, remarks on the traces left in organic structure by the form of action which the disease constitutionally or casually assumed.

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* Dr Borland was requested by the Director-General to prepare the above sketch, in consequence of having passed many years of service with Dr Jackson, and enjoying more intimate friendship with him than any other officer of the department. The hasty manner, however, in which it was drawn up, only a day or two before the annual meeting, renders it unavoidably more meagre than Dr Borland would have wished, and the merits of Dr Jackson may seem in the eyes of his friends to demand.

The scene of his observations in Jamaica was comparatively narrow. The war in North America opened to him a field more extensive; and impelled by that zeal to enlarge the sphere of his knowledge, which glowed with unabated fervour to the last day of his existence, he volunteered service in America in the year 1778; and soon after his arrival was attached to the late 71st regiment; in the various services of which corps he had the opportunity of observing very extensively the different forms and degrees of fever in the southern states of the North American continent. The result of his experience in Jamaica and America was submitted to the public in 1791, entitled "*A Treatise on the Fevers of Jamaica, with Observations on the Intermitting Fever of America; and an Appendix, containing some hints on the means of preserving the health of soldiers in hot climates.*" The materials of this admirable clinical treatise were collected between the years 1774 and 1782. The interval between his return from America in 1782, and its publication in 1791, a period of nine years, was devoted by Dr Jackson to supply valuable additions to an originally scanty stock of academical knowledge; for he candidly confesses in his writings, that, from want of financial means, his early professional education had been very imperfect. As there are few obstacles, however, which an ardent mind will not surmount, he diligently entered upon, and successfully completed a most laborious course of mental discipline during this period, which was passed partly in study at Paris, —partly at the University of Leyden, where he took his doctor's degree,—partly in travelling over a great portion of Europe, surveying minutely the medical, military, and literary institutions of foreign states; and, finally, at Stockton-upon-Tees, in the county of Durham, where he succeeded to extensive practice, and was held in high estimation.

At the beginning of the French revolutionary war in 1793, desirous of investigating still farther the subject of tropical fever, Dr Jackson offered service with an expedition about to proceed to the West Indies, under the command of General Sir Charles Grey; but not possessing the qualifications required by the regulations of that day, for the commission of physician to the forces, he accepted the surgency of the 3d regiment of foot or the Buffs, then embarked for the West Indies. The destination of the corps, however, being changed on the eve of sailing, he accompanied the regiment to Flanders, where he was soon after appointed acting physician to the forces by his Royal Highness the Duke of York, who commanded the army serving there. The appointment was not confirmed by the ruling authorities at home; but the doctor's talents and exertions, under trying circumstances, during a very harassing retreat, became so conspicuous, that, on the demise of Dr Kennedy, he was at once raised to the rank of chief inspector, and continued to superintend the military hospitals until the British troops were withdrawn from the continent. The change of his destination from the West Indies to Flanders was considered by Dr Jackson a great disappointment at the time, since it interrupted the researches he then had in view, upon an extensive scale, into the origin, progress, and issue of the diseases of the tropical regions. But the event ultimately turned out to be fortunate, both for his own satisfaction and for the advancement of science, as it afforded him the opportunity of investigating, in a wide and varied field, the not less important subject of contagious fever, which prevailed to great extent during the retreat of the British troops through Holland into Germany; of the medical history of which campaign, memorable for its sickness and mortality, his pen has presented us with a melancholy but accurate and instructive picture.

In the year 1795, immediately upon his return from the continent, and before he had time to publish his observations on contagious fever, Dr Jackson was ordered to the Island of St Domingo, where the duty assigned to him afforded the means of examining the features and causes of endemic disease, at different posts, and in different climates, more fully than happened to any other person on the medical staff of that island. The accustomed rule he had pursued in Holland of writing histories of diseases at the bedside of patients, and minuting every point connected with the influence of local agencies in the production of sickness, was again assiduously observed in St Domingo, although many other laborious duties, as inspector and head of the department, had devolved upon him.

He returned home in 1798, and in the same year published the results of his previous experience on the continent of Europe, and of his more recent and fatiguing researches in St Domingo. This work bears the title of "*An Outline of the History and Cure of Fever, Contagious and Endemic; more expressly the contagious fever of ships, jails, and hospitals; and the concentrated endemic, vulgarly called the yellow fever of the West Indies; to which is added an explanation of the principles of military discipline and economy; with a scheme of medical arrangement for Armies.*"

The superintendence of the medical concerns of a body of Russian troops, consisting of 17,000 men, which acted with the British army in the expedition to the Helder in the year 1799, was next committed to Dr Jackson by the desire of the Russian ambassador, Count Worronzow.

These troops, after the termination of their disastrous service in North Holland, were stationed in the islands of Jersey and Guernsey, where they arrived in a most disorganized and sickly condition; and the management of their numerous hospitals was conducted upon a principle of efficiency, economy, and judicious adaptation of means to purposes, that had not hitherto been witnessed in the British army.

The success that attended his managements on this special service was so manifest, that his Royal Highness, the late lamented Commander-in-Chief, was induced the following year, 1800, to appoint Dr Jackson to the situation of physician and head of the Army Depot Hospital at Chatham, afterwards moved to the Isle of Wight, where he had many difficulties to overcome in introducing an improved system of hospital improvement. Circumstances, however, having unfortunately caused a want of confidence and cordiality between him and the physician-general and the surgeon-general, especially the latter officer, who was the organ of official correspondence, Dr Jackson felt he could not continue to serve satisfactorily to himself, nor, as he conceived, usefully to the public. He therefore applied for leave to relinquish the situation which he held; and the official letter from the military secretary to his late Royal Highness proves that he retired without a stain upon his honour or integrity.

Thus divested of official employment, Dr Jackson retired to his former residence at Stockton, resuming the exercise of his profession in civil life to the great satisfaction of the inhabitants. He found leisure, however, in 1803 to publish "*Remarks on the Constitution of the Medical Department of the British Army, with a Detail of Hospital Management; and an Appendix attempting to explain the Action of Causes in producing Fever, and the operation of Remedies in effecting Cure.*" And this work was followed in the year 1805 by "*A System of Arrangement and Discipline for the Medical Department of Armies,*" dedicated to the medical officers of the British army. In both of these interesting publications much useful instruction was conveyed, especially to the junior class of medical officers.

In the year 1810 Government instituted an important change in the constitution of the army medical department. The board, hitherto consisting of a physician-general, surgeon-general, and inspector-general, was dissolved; and the arrangement was vested in a director-general, assisted by two principal inspectors, upon a plan analogous to that adopted in other extensive military departments This change opened the door to Dr Jackson's return to active service without any sacrifice of his feelings; for it was founded on the very principles he had strenuously advocated. Accordingly, in the year 1811 a vacancy having occurred in the direction of the medical concerns of the Windward and Leeward Island station, this enterprising servant of the public, notwithstanding his previous toils in the torrid zone, again came forward, with unimpaired talents and undiminished zeal, to offer service, which was accepted by the late director-general. He immediately proceeded to Barbadoes, the head quarters, pursuing a most active superintending duty for several years, assisting at intervals all the islands and colonies within that military command, and finally contributing to the records deposited in the director-general's office, a most valuable series of health inspection reports, comprising the medical topography of every island and station occupied by British troops in the West Indies, including Jamaica, to which island his inspection was also extended under a special commission. In 1815 he returned to England, and in 1817 presented to the public the sum of his observations during this recent service, in " A Sketch of the History and Cure of Febrile Diseases; more particularly as they appear in the West Indies among the Soldiers of the British Army." Of this work a second edition, in two volumes, was published in 1820, containing, besides other additions, a most valuable summary of his official reports on military positions, barracks, and hospitals in the West Indies, which had been submitted officially to the commander of the forces on that station between the years 1812 and 1815, with the view of directing the attention of government to the best means of preserving the health and lives of the military in that climate, and at the same time of husbanding the pecuniary resources of the nation. Of this edition the following notice was taken in the bibliographical record of the quarterly Journal of Medical Science.—" The matter indeed of these two volumes offers a most astonishing fund of information on the subject of fever, and no tropical visitor in particular should proceed to his destination without possessing the work. The European physician, too, will find that the veteran Jackson has anticipated almost every modern writer on fever in all those points of pathology and practice in which we excel our forefathers."

Although Dr Jackson had become perfectly familiar with the disease, vulgarly named yellow fever, in the West Indies, having, at three distinct periods of his life, possessed extraordinary official opportunities of observing it in all its forms and gradations, in every variety of position, with a keenness of research peculiar to himself, as yet no opportunity had been afforded him of seeing the epidemic malady, known by the same name, which has ravaged the sea-coasts of the south of Spain at intervals since the beginning of the present century. But in the autumn of 1819, this epidemic having been reported to have made its appearance at Cadiz, Dr Jackson, anxious to know its nature, and conceiving that it might be an object of some importance to the nation to ascertain whether or not it was contagious, made an offer to the government, through the present director-general, to undertake the investigation of the subject, and to proceed to Cadiz without loss of time. This zealous act of devotion to the interests of humanity having been submitted to the secretary of state, the offer was accepted; and such forms of introduction having been obtained, after some delay, from the Spanish Ambassador as would secure protection from the Spanish government, and attention from the local authorities of the districts where the disease prevailed, Dr Jackson repaired to Gibraltar. But when in the act of setting out from that garrison for Cadiz to commence his researches, information was received by the governor of the breaking out of the Spanish military insurrection, and the consequent interruption of the route from Gibraltar to Cadiz. Thus defeated in his immediate purpose, detained inactive at Gibraltar, and seeing no prospect that the route to Cadiz would be again open before the fever ceased to rage, he undertook a journey to the Levant, on his own motives and at his own expence, partly with the desire of seeing Greece,—a country hallowed by classical recollections, and partly in the hope of obtaining information respecting the plague—a disease he had never seen, and concerning the nature and history of which he could form no correct idea from what he had read.

In prosecution of this plan he went from Gibraltar to Malta, to Constantinople, to Smyrna,—and from Smyrna passed among the islands of the Archipelago to Athens and the Morea, finally embarking at Patras for Zante, where he arrived in June 1820. The plague did not prevail in the Levant during the time of the doctor's excursion; but, as investigation into the nature of the yellow fever was his motive for leaving England, he determined to be at Cadiz in August; and therefore embarked at Zante for Gibraltar, which fortress he again reached the latter end of July. His health had suffered seriously during this excursion; but, nevertheless, he set out from Gibraltar to Cadiz on the 23d August, and, arriving on the 25th, it so happened that the appearance of the disease was announced the very day he entered the city. The fever was epidemic for two months at Cadiz, where he remained until he satisfied himself of its character; but, hearing that the malady which raged at Xeres was more severe, and of greater proportionate mortality, he went thither to ascertain its real condition. A statement of his opinions, with a detail of his investigations at both places, in which he was assisted by an intelligent medical officer, whom General Sir George Don kindly permitted to accompany him from Gibraltar, was published the following year, 1821, under the title of "*Remarks on the Epidemic Yellow Fever which has appeared at intervals on the south coasts of Spain since the year* 1800." This the last, though not the most toilsome of our lamented friend's benevolent career, is here more enlarged upon; because it is impossible not to admire the sacrifice of all personal convenience and domestic comfort, and the grappling with danger in the cause of science, in a pursuit prompted by the most generous motive that can influence the heart of man, voluntarily undertaken, and perseveringly prosecuted, at the age of three score and ten, with an ardour seldom surpassed in the vigour of youth. But your admiration at his zeal will be augmented when you are informed, that, only a few weeks before his death, in his 77th year, he conveyed to the director-general an offer to waive his rank, and to proceed to Portugal, to do duty as physician in the military hospitals under the control of a junior.

Dr Jackson died on the 6th of last month at Thursby, near Carlisle, while occupied in revising and condensing into systematic form the whole of his manuscripts on the subject of fever; and if the work be sufficiently advanced to show to others the plan and intention of the author, a hope may be entertained of its publication by competent authority.

It was not intended, nor would it be possible, in this place, to present to you even a faint outline of the various events of Dr Jackson's long and active life; but it is presumed, that, from materials believed to be in the possession of his widow, should they be entrusted to some friend qualified for the task, a biographical sketch might be prepared that would attract notice from the public, as well as from the medical reader. To the military reader Dr Jackson has bequeathed a most interesting quarto volume, namely, "*A View of the Formation, Discipline, and Economy of Armies,*"—the first edition of which was published in 1804, and the last edition, containing the result of twenty years' additional observation, gathered in a most ample field, was presented to the public in 1824. This able work, philosophical in its design, containing sketches of the military character of different nations, drawn with great spirit, fidelity, and discrimination, would elevate the author high in the ranks of literature, even if it had been his only contribution to the republic of letters; and it is no trivial commendation of it to state, that, after the late gallant General Sir John Moore had read the first edition of this work, he wrote to the author in the following terms:—"My dear Jackson,—I have perused your military book, with which I am much delighted. There are none of us, even the most experienced, who may not derive instruction from it; and I only regret that you who possess a soldier's mind had not been a soldier by profession."

Dr Jackson's indefatigable perseverance to obtain a contemplated result from every pursuit in which he engaged may be illustrated by his having commenced the study of the Arabic language after his return from America, with a view to the fullest investigation from original sources of his favourite subject, fever; and, notwithstanding various interruptions, he very soon attained such perfect knowledge of it as to be able to read with facility, and understand thoroughly the writings of all the old eastern physicians.

His curiosity at one time was much excited by the controversy respecting the authenticity of the poems ascribed to Ossian. Although not a Highlander, (for he was a native of Lanarkshire, a lowland district of Scotland,) Dr Jackson had long served in a Highland regiment, was much attached to Highlanders, and felt interested in the question at issue, which peculiarly belonged to the history of that mountain race; but, being ignorant of their language, he could not venture to settle the disputed point in his own mind. He, however, eagerly entered upon the study of the Celtic tongue to qualify him to do so; and having mastered the Gaelic language, and having listened attentively to traditional informations drawn from conversations with the old Highland people, he came to the conclusion, that the poems, though but imperfectly translated, were genuine, and not impostures fabricated by Mr M'Pherson, as had been asserted by Dr Samuel Johnson and others. His opinion will be found by those who are curious on this subject at page 263 of his military work.

When the evacuation of Port au Prince in St Domingo by the British troops caused a reduction in the staff, and Dr Jackson and several other medical officers were preparing to return to England, one * of them suggested to him a joint excursion of amusement through the United States of America, a plan to which he readily, indeed eagerly, assented. They accordingly embarked at Mole St Nicholas; and General Maitland having had the goodness to furnish them with letters of introduction, and dispatches to Mr (now Sir Robert) Liston, then British minister to the United States, they arrived at Philadelphia, where they met with marked attentions and much hospitality from the celebrated Dr Rush and other American physicians. And to Dr Jackson it must have been gratifying to find that his professional fame had preceded him in that country, his first book on fever being found in the hands of every respectable practitioner.

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* Dr Borland.

This tour furnished instruction, as well as recreation, affording an opportunity, which he did not fail to seize, of investigating the nature of the yellow fever which then prevailed at Philadelphia and at New York. He had also the satisfaction, in the former city, of renewing his acquaintance with the American General Morgan, whose prisoner he had formerly been during the revolutionary war, under circumstances so highly honourable, both to the conqueror and the captive, that I shall here relate the particulars.—" During the heat of an action fought at Cowpens by a division of the British army, under disadvantages of unfavourable position and numerical inferiority, at a moment when the unfortunate issue of the battle was no longer doubtful, Dr Jackson, who happened to be well mounted, perceiving that the horse of the officer commanding the British troops had been shot under him, immediately rode up to the dismounted commander, and tendered to him the horse he was riding, observing, that he (the doctor) was but an obscure individual, whose escape could have but little beneficial influence, but that his (the officer's) safety was of the highest importance to the army. The commander accepted the generous offer and escaped. Dr Jackson, with some other officers and men, and all the wounded fell into the hands of the enemy. When a prisoner, he continued not only to give the benefit of his kind aid to the British soldiers, but extended his professional assistance to the wounded Americans; a conduct that particularly attracted the notice of the American general, who handsomely marked his sense of it by restoring the doctor to liberty, without even exacting his parole." This anecdote, so truly characteristic of the man, and evincing traits of heroism and generosity deserving of record, was not communicated by Dr Jackson, but by a military officer who was in the action; and when the doctor was afterwards referred to, in terms of surprise, that he had never mentioned it to his intimate friends, and was pointedly questioned as to its authenticity, his short reply was, " It is true."

Those who best knew Dr Jackson loved him most. Simple and unpretending in his habits and manners; independent in mind; bold in the pursuit of truth, but modest in the expression of it, he went about continually doing good in his sphere. He was a liberal contributor to the improvement of this department; a warm friend to the soldier; a benefactor to the human race; and had he been born to hereditary fortune, or been blessed with wealth, he would have moved in the same path with the disinterested and philanthropic Howard. His body has returned to dust. His spirit has departed hence. But the name of Jackson will live so long as medical science shall endure. J. B.

Art. XIII.—*Notes of a Case in which the Canal of the Larynx after being nearly obliterated, was re-established.* By Robert Liston, Esq. Surgeon, Lecturer on Surgery, &c.

Elizabeth Oswald, æt. 27, in attempting suicide in September 1826, had wounded the larynx through the crico-thyroid ligament.

She was under treatment for some months, and was at length abandoned with the loss of voice, and breathing entirely through a silver tube introduced in the situation of the original wound. On withdrawing this tube, and examining the parts attentively, a very minute orifice, capable of admitting the point of a probe of half the usual size, was discerned leading towards the glottis

About April 15, 1827, Mr Liston saw her by the request of Mr Sidey, who had attended much to the case from the first, and began to introduce bougies about the size of darning needles into the part of the trachea above the wound; and by gradually increasing the size of the bougies the passage was brought about the end of June to its natural diameter. About the middle of August the lower part of the trachea was dilated by a tube gradually increased in size, so that one of sufficient diameter might be passed by the mouth.

Three weeks afterwards the short tube was removed, and a long one was introduced by the wound up into the mouth, there laid hold of and pushed down into the trachea. This was followed by a very severe fit of coughing, which lasted about half an hour.

This tube, which was about nine inches long, and equal in diameter to that of the largest œsophagus tubes, was retained in the windpipe for fifteen days, during which it caused great salivation, which loosened the teeth and extremely reduced the strength of the patient. In consequence of this it was necessary to use some active means for the closure of the wound. On the fifth day the cicatrix was dissected out by two elliptical incisions, and the edges of the wound united by suture. The tube being removed on the fifteenth day, (October 5th,) she breathed well. In the morning, however, her breathing became very difficult, and *tracheotomy* was performed, and a silver tube introduced, which was kept in five days, and then replaced by a smaller one.

The actual cautery was applied to the edges of a small sinus, which remained on October 17th; and on the 26th it was reapplied to the opening, which had very nearly closed. On the 28th the tube was removed; and a few days after the wound was completely closed. The woman now breathes with comparative ease through the larynx, and is slowly recovering the use of her voice.

PART II.

CRITICAL ANALYSIS.

Art. I.—*Lectures on the Operative Surgery of the Eye; or an Historical and Critical Inquiry into the Methods recommended for the Cure of Cataract, for the Formation of an Artificial Pupil, &c. &c. &c.* By G. J. Guthrie, Esq. 2d Edition, with Seven Explanatory Plates. London, 1827. 8vo, pp. 554.

In the cultivation of ophthalmic surgery the surgeons of this country have been rather behind their foreign brethren. The diseases incident to the eye, and the means necessary for their cure, had been assiduously investigated by the French and German authors long before they had attracted much attention in England. With the exception indeed of the obscure treatises of Coward, Read, Crosse, and others equally unknown, the desultory writings of Taylor, and the obsolete book of Chandler, the English literature of surgery possesses no work professedly on the diseases of the eye; and only in the writings of Cheselden, Sharp, Warner, Pott, and Wathen, do we find some incidental observations on the merits of certain modes of treating cataract, and some of the more ordinary complaints of the organ of vision.

The first of the British surgeons who made the diseases of the eye a particular study were Ware and Cunningham Saunders; and even the writings which these ingenious men have left can by no means be compared with those, especially of the German surgeons. To omit the labours of De Berger, Heister, and Richter, which belong to an earlier era, the literature of Germany boasts of the elaborate writings of Schmidt, Beer, the two Wenzels, Soemmering, Himly, and more recently those of Benedict, Walther, Weller, and Langenbeck; while that of Italy may justly be proud of the classical production of the Professor of Pavia.

On the career of these distinguished models, however, our countrymen have lately made rapid and effectual advances. The writings of Wardrop, Lawrence, Vetch, and Travers, have contributed in a remarkable degree to illustrate the nature of many of the diseases incident to the organ of vision, and have principally tended to introduce a more regular and systematic method of cure than had been in previous use. The example of these experienced surgeons, combined with the establishment of a variety of charitable institutions for the express purpose of affording relief to those of the lower ranks labouring under different forms of impaired vision, has procured for the study of ophthalmic surgery a degree of attention and interest from which, doubtless, the art of the oculist must be justly expected to derive the greatest improvement.

In this department, however, the most zealous labourer is undoubtedly Mr Guthrie; and to him the English student is under the greatest obligations for the diligence with which he has exerted himself to render the art of the oculist a branch of surgery at once scientific and accessible. The present work of this author is devoted chiefly to the operative branch of ophthalmic surgery; and on this department is it doubtless the most comprehensive treatise at present extant. Though enjoying excellent opportunities of acquiring practical information on the diseases of the eye requiring the interposition of the surgeon, Mr Guthrie has not confined himself to the results of mere personal experience. He has evidently bestowed great pains in studying the works of the foreign ophthalmological writers, and he has exercised in general a just and liberal spirit of remark on the merits of their respective methods of treatment.

The diseases of the organ of vision requiring the aid of surgical operation consist, as in other parts of the body, of certain changes in structure, which are very often the effect of inflammation, or some similar disorganizing process. When they are the consequences of the former agent, they may often be prevented, or at least rendered less injurious in their effects by the prompt and timely interposition of general medical and surgical treatment. When they have exceeded this point, and such a result may often take place, they are no longer amenable to the same influence; and nothing short of an operation particularly adapted to the purpose is adequate to restore to the organ its former utility. In other circumstances organic changes may take place slowly and insensibly, yet with great certainty, and either impair the utility of the organ, or destroy it entirely. In this case also the surgeon has no resource, save a judicious and dexterous application of the principles of his art.

Entropion, or inversion of the eyelids, whether occurring in the upper or in the lower eyelid, depends, according to Mr Guthrie, on the conjunct operation of several causes. An opinion has very generally prevailed, that this painful and obstinate complaint depends on the mere inversion of the hairs of the eyelashes. Mr Guthrie shows clearly, that, though this inversion is part, it does not constitute the whole of the disorder; and the derangement of the eyelashes, termed *trichiasis*, is itself a consequence of disease,—generally chronic inflammation of the eyelid and tarsus. Inversion of one or more *cilia* may result from various causes, and produce a partial inversion of the eyelid; but complete general inversion or entropion is a consequence of chronic inflammation, operating at once on the tarsal cartilage, and on the excessive action thereby induced in the *orbicularis* muscle.

He adopts that part of the hypothesis of Mr Crampton which ascribes this vicious direction of the eyelids to the contractions of their angles; but objects to the competency of that which supposes the disease to depend on the shrivelling and thickening of the tarsal conjunctiva, for two reasons. 1*st*, In the most severe cases of granulated or fungous conjunctiva, instead of inversion, there is a tendency to eversion; and 2*dly*, as the tarsal conjunctiva is firmly attached to it, the action of the levator palpebræ must be on the whole, and not on the edge, as the hypothesis of Mr Crampton requires.

In short, general inversion of the edge of the tarsus of the upper eyelid Mr Guthrie represents to be first determined by the swelling of the external parts in inflammation, acute or chronic, and the spasmodic and extreme contraction of the *orbicularis*, and to be completed by contraction of the angles of the lids, and the inversion of such hairs as are matted in the vitiated discharge of the conjunctiva and Meibomian glands.

To this view of the pathology of entropion must be conceded the merit of being more comprehensive in the consideration of the several causes than any which has preceded it. If we could venture to offer any suggestion to an oculist of the great experience and accurate observation of Mr Guthrie, we should say, that in explaining the phenomena of inversion, not only the circumstances enumerated by him are to be taken into account, but more especially the physical properties and the mechanical configuration of the tarsal cartilages. Every case of entropion is preceded by inflammation of these bodies, the effect of which is to increase considerably their natural hardness and firmness. The inflammation also causes excessive contraction of the ciliary part, especially of the *orbicularis*, which, however, would be of little moment, but for the firmness and the semicircular shape of the tarsal cartilage. Such is the operation of this contraction under these circumstances, that after the eyelids have been forced to their utmost over the eye-ball, while the two extremities can be moved no further, the intermediate circular segment of the tarsus is all at once made to describe a circular motion inwards and upward on the eye, the centre of which motion is a point intermediate between the two extremities of the tarsus and the origin or acting point of the ciliar muscle.

That this takes place may be clearly proved by the examination of a few cases of the disease, in which the middle part of the tarsus is always found more inverted than the two extremities. That the motion depends on the increase of the natural firmness and the semicircular shape of the tarsus is obvious to every one who understands the laws of mechanics, and may be readily illustrated by observing the motion which takes place in a rigid elastic hoop, the two ends of which are fixed, while the central portion is under the operation of a moving power.

The same circumstances account for the fact remarked by all practical oculists, but by no means satisfactorily explained, that entropion is infinitely more frequent in the upper than in the lower eyelid. The former is more extensive, or is the segment of a much larger circle than the latter, and hence is much more readily operated on by the orbicular muscle, and much less easily restored to its natural direction. It is only in this manner also that can be understood the vicious bending and curvature of the cartilages so strongly insisted on by Mr Saunders, and also by the present author, and the contraction of the angles to which the disorder is ascribed by Mr Crampton. This vicious curvature is the joint effect of morbid induration of the tarsi, bodies naturally firm and unyielding, and the internal projection occasioned by the excessive action of the orbicular and ciliar muscles. When inversion is once completed, it is easy to see how difficult it must be to restore the cartilage to its original position.

In considering the treatment of this disease, Mr Guthrie enumerates the methods which have been in use at different periods from the time of Galen, Antonius Musa, Celsus, and Paul of Aegina, down to Heister, Maitre Jan, St Yves, Scarpa, Monteggia, and the recent German oculists. Saunders, it is well known, one of our most ingenious and rational oculists, declared that in the inveterate and established form of entropion nothing short of extirpation of the tarsus is effectual. But Mr Guthrie has had occasion to witness living monuments of the insufficiency of this measure, even in the persons of patients on whom Mr Saunders had himself performed this operation. A more effectual remedy he has found in modifying the method originally practised by Mr Crampton. This consists in dividing perpendicularly by the knife or the blunt-pointed scissors as much of the eyelid close to the external angle as renders it perfectly free, and in the same manner at the inner angle, but without including the *punctum lacrymale.* When these incisions are made to the requisite extent, the elasticity of the tarsus, no longer confined by the bridling effect of the angles, may be so considerable as at once to return the eyelid, tarsus, and eyelashes to their natural position. Where this does not take place, either from lateral adhesion, or from extreme induration and habitual curvature of the tarsus, the attachments should be divided, and the incisions prolonged, and the part of the eyelid included between them must be completely everted; and it may be even requisite to divide the tarsal cartilage through its most indurated and contracted point. Of the division of the conjunctiva inculcated by Crampton, Mr Guthrie disapproves as unnecessary, and even injurious.

When these essential parts of the operation have been completed, a fold of the skin of the eyelid between the incisions should be raised by the forceps, and cut away by the curved scissors; the requisite ligatures are to be inserted, and the usual dressings to be applied. The details of the after-treatment may be found in the work itself.

The effects of this operation are illustrated by the relation of six cases, in which its success was complete.

In slight cases the application of concentrated sulphuric acid, as originally practised by Helling of Berlin, and afterwards by Quadri of Naples, may be sufficient to effect a cure.

Ectropion, or eversion of the eyelids, is much more common and much less serious, and is rather a complaint unsightly to the spectator, and annoying to the patient, than productive of much real distress. In a slight degree it is exceedingly common as a consequence of chronic inflammation, when the eyelids are villous, or granular, or hard as cartilage. It may occur in the young after long-continued or repeated attacks of palpebral inflammation, often called strumous, and certainly occurring chiefly in strumous habits. It may take place in the adult under the same circumstances. And in the aged it is frequent as an effect of repeated inflammation of the conjunctiva, where great relaxation of the parts combines with a loaded state of the vessels to prevent restoration to the sound state. In these several forms it constitutes the *lippitudo* of the Romans.

In explaining its etiology, Mr Guthrie directs the attention of his reader, not only to the swelled state of the palpebral conjunctiva, the diseased Meibomean glands, the successive decision of the eyelashes, and the morbid secretions issuing from these parts, but to the excoriation, hardening, and shrivelling of the skin, produced by these irritating secretions. The fungous state of the conjunctiva, on which Scarpa has laid great stress, is neither an essential cause of eversion, nor when present always produces it. The contracted indurated state of the skin of the eyelid is, on the contrary, invariable; and between the degree of this change and that of the eversion a very exact relation may always, according to Mr Guthrie, be traced.

In this representation Mr Guthrie may be right; and it would be improper to deny unconditionally the accuracy of his observation with regard to the influence of the shrivelled skin in producing ectropion. We cannot refrain, however, from expressing the opinion, that Mr Guthrie, in his anxiety to assign to the skin an important part in the formation of this complaint, overlooks, as he does with regard to entropion, the influence of the physical properties and shape of the tarsal cartilages. It is to be remarked, indeed, that nothing tends so readily to evert the tarsus as the swelling of chronic palpebral inflammation; that when once this swelling attains a certain point, the same circumstances which we noticed produce inversion, will now operate with equal facility in producing eversion; that even the simple action of shutting the eye forcibly in this state causes eversion; and that the shrivelled state of the skin is to be viewed rather as an effect than a cause of the disease.

The justice of these observations is, we conceive, strongly evinced in the second sort of ectropion mentioned by the present author,—that resulting from acute inflammation. It is matter of notorious observation, that in acute purulent ophthalmy, both of infants and adults, nothing is more common than more or less eversion of the eyelid, which subsides in general, unless the conjunctiva has been rendered villous and granular. In this case the eversion is permanent, and requires the use of remedies.

The only instance in which the contraction of the skin can be justly admitted to be an essential cause of ectropion is in that of the third species of Mr Guthrie,—the eversion in consequence of the cicatrix of a wound or sore in the immediate vicinity of the eyelid. The mechanism of this process, and the circumstances under which it occurs, are so well understood to the surgeon, that it is unnecessary to take up the time of the reader with the details, which may be read in the work of Mr Guthrie.

A fourth form of the complaint is that which depends on the conjunct effect of palsy and slight chronic inflammation of the palpebral conjunctiva. What muscles are affected in this palsy Mr Guthrie does not mention. It is manifest that the lower eyelid, in which chiefly this variety of the disease occurs, is connected with no muscle which could produce the effect alleged; and that the only loss of power which can be physiologically admitted in this case, is that relaxed and flaccid state of the parts, which is nearly uniform at an advanced period of life.

With the distended, varicose, and often villous state of the palpebral conjunctiva, this is amply sufficient to produce that peculiar haggard eye which has been truly said to be "quenched in the rheums of age."

The treatment of the several varieties of ectropion is regulated on much the same principle, and effected nearly by the same means. In laying down his indications, Mr Guthrie allows himself, in our mind, to be misled and embarrassed by his hypothesis of cutaneous contraction; and in his first species the first indication is to relieve the contraction of the skin; and the second, to return and retain the lid in its proper situation. If the skin were really shrivelled, we fear the zinc ointment could not effectually relax it. It is chiefly to the treatment of the diseased conjunctiva that the replacement of the tarsus is to be ascribed. Mr Guthrie recommends the method by the concentrated sulphuric acid applied to the everted surface of the eyelid. After six or eight applications, at the interval of four or five days, of this remedy, which has the effect of cauterizing the diseased surface, which is then thrown off by sloughing, the thickening of the conjunctiva is much diminished, and the tarsus is considerably retracted to its natural position. The cure may be then completed by the use of any of the stimulant ointments.

In the employment of the acid, care must be taken not to allow it to touch either the eye or the ocular conjunctiva, and not to allow it to proceed so far as to induce inversion, which has happened.

In the second form of ectropion the villous state of the conjunctiva is best removed by the frequent, but light and careful application of the lunar caustic. When it is the consequence of acute inflammation, and consists chiefly in loaded vessels, in which the blood stagnates, and is prevented from returning by the partial strangulation of the eyelid, scarifications and mild astringents are the proper remedy.

Ectropion resulting from the contraction of a scar is very difficult to remove. Mr Guthrie shows that the cicatrix must be extirpated, and a new one by granulation must be formed. The principle to be kept in view in this case is the same as that on which Mr Earle operated in relieving the unsightly contractions which result from badly-healed burns.

On eversion of the upper eyelid, or the hare-eye (*lagophthalmos,*) a much less frequent complaint, Mr Guthrie delivers some judicious observations.

Relaxation of the upper eyelid (πτωσις, *blepharoptosis, atoniablepharon*) depends on loss of power, congenital or acquired, in the *levator palpebræ superioris.* Mr Guthrie ascribes it also to œdema, to relaxation of the skin, or to an atony of the eyelid. Perhaps, if it cannot be traced to palsy, to some of the premonitory symptoms of apoplexy, to hysteria, or to epilepsy, it may always be referred to that nervous, or rather relaxed and atonic state of the system which results from disorder of the alimentary function. In such circumstances the proper remedy is the removal of the disease of which it is a symptom.

On the subject of *adhesion of the edges of the eyelids* and *wounds of the eyelids* it is unnecessary to dwell. Under the latter head Mr Guthrie enters at some length into the consideration of that variety of amaurosis which occasionally succeeds injuries and wounds of the contiguous parts.

It has been generally imagined, that, when loss of vision, with dimunition of the transparency of the cornea and humours, succeeds wounds or other injuries in the neighbourhood of the organ, it is caused either by concussion of the organ or by some mysterious nervous operation. Mr Guthrie thinks, however, that defective vision, or complete privation of sight, rarely takes place, unless in consequence of injury of some nervous branch sufficiently considerable to admit of demonstration by dissection. These wounds or injuries may be situate either on the forehead, towards the nose, implicating the supra-orbital branch, and the nasal branch of the first division of the fifth pair,—or immediately below the eye, affecting the second division of the same nerve. It is not necessary that injury be inflicted on the eye itself; and it is well established that the impaired vision is the sympathetic effect of injury done some of the ophthalmic system of nerves. It is remarkable that the loss of vision is almost never immediate. In most instances it is observed to approach slowly during the process of healing; and in some it appears only when cicatrization is complete.

The mode in which these injuries operate is understood by reflecting on the arrangement and distribution of the ramifications of this ophthalmic division of the fifth pair. The researches of J. Frederic Meckel and of Zinn showed that the white radiated fibres in the uvea or posterior part of the iris consist chiefly of nervous filaments, which proceed from the lenticular or ophthalmic ganglion; and as this body is formed principally of nervous twigs from the first branch of the fifth pair, the influence of its integrity or its lesion over the functions of the iris is obvious.

In some few instances the deprivation of sight is said to be instantaneous; and in these Mr Guthrie is of opinion that the eye itself has suffered from the general concussion,—that is, we imagine, from the shock inflicted on some part of the bony orbit. That this opinion is well founded will be readily admitted by those who know with what facility a blow inflicted on a hard body as bone is propagated from the point of percussion to one more remote. Thus, a blow inflicted on any part of the orbit from the external orbitar to the internal orbitar process, or even on the lateral part of the frontal bone, may follow such a direction as to expend the sum of its force somewhere in the bottom of the orbit, and affect severely the choroid and retina. The first effect of this may be analogous to the first effect of a similar blow on the head,—viz. concussion more or less severe, and consequent derangement or interruption of function, or even hemorrhage from lacerated vessels. In the latter case more or less extravasated blood is generally seen in some part of the posterior chamber, or in the vitreous humour. The second effect is quite similar to the secondary effects of blows on the head, viz.—a degree of inflammation more or less severe and more or less distinct. It will depend very much on the sagacity of the practitioner, and on the activity and promptitude of the subsequent treatment, whether the choroid and retina are not permanently and irremediably injured.

This, we conceive, is the true theory of impaired vision supervening immediately or soon after blows on the orbit, without demonstrable injury of nervous filaments. It also affords the explanation of the fact, that, in some instances the vision is abolished only in part, or in one-half of the organ. It finally suggests the appropriate treatment,—a point on which Mr Guthrie deems the interposition of the oculist as unavailing. It is manifest that the injury ought to be treated on the same principles as the effects of injury or violence in general.

We must not omit to notice, nevertheless, that the present intelligent surgeon is by no means entirely unaware of the true nature of the consequences of this species of injury. He justly remarks, that every wound of the forehead implicating the fifth pair of nerves is not succeeded by amaurosis, and conversely, that many cases of defective vision accompanied by wounds depend on other injury than that inflicted on the forehead. He seems to think that the injury may have been inflicted directly on the eye, in which extravasation or marks of laceration are sometimes manifest. But even this supposition, as we have shown, is unnecessary; for violence on the forehead, without any injury of the nerve or eye, is perfectly competent to produce the effect in question.

Mr Guthrie gives no encouragement to the practice of Beer, which consists in dividing the nerve down to the bone, in cases supposed to depend on injury of the ophthalmic branch of the fifth pair. It is easy to discover the real cause of the favourable results obtained by this oculist, and the want of success in the hands of Scarpa, Guthrie, and others. The oculist of Vienna divided the nerve at once, without knowing whether in any given case the amaurotic blindness was to ensue. Now, as Mr Guthrie has justly observed, not every case of nervous injury is followed by defective vision; and to this it may be added, that in several, in which vision becomes impaired it is spontaneously recovered after some time without known cause. On this head we may refer the reader to some good observations by Benedict in his eleventh book on Traumatic Ophthalmia.

In the chapter on tumours a good deal of miscellaneous matter is introduced. The pathology of stye is correctly stated; but it appeared to us unnecessary to refute the erroneous notion, that this disease consists in inflammation of a Meibomian gland. In this meridian, at all events, Mr Guthrie may be assured, it was never understood to be anything else save inflammation of the cellular tissue between the delicate skin of the eyelid and the tarsal cartilage. Conversely, we think *chalazeon* is very often, if not always, an enlargement of a Meibomian gland. On the removal of encysted tumours, which are the same in the eyelids as elsewhere, the reader will consult the work itself.

Encanthis, it is well known, consists in enlargement of the *caruncula lacrymalis*, implicating also the semilunar valve, and spreading to greater or less extent along the edge of either eyelid. This disorder Mr Guthrie justly views as the result of inflammation ill treated, neglected, or passed into the chronic state. Of the inveterate stage and cancerous form described by Scarpa, he repeats the description of that author.

In treating this disorder he recommends antiphlogistic means in the early stage and inflammatory form, the use of the scissors in the case of excrescences or mulberry growths, and the extirpation by ligature or the knife when it is large and likely to prove malignant.

On the nature of *Pterygium* he differs from Scarpa entirely. This eminent pathologist, it is well known, regards *pterygium* as an effect of chronic ophthalmy, and as consisting in a relaxed varicose state of part of the conjunctiva. From very great experience in the treatment of chronic inflammation, as also from the observation of several friends who have examined the pensioners discharged in consequence of diseases of the eye, Mr Guthrie thinks he is justified in doubting the soundness of this opinion; and he further infers, that not only is true pterygium rarely the consequence of inflammation, but that it differs essentially from those nebulæ of the cornea with which Scarpa has classed it. Mr Guthrie, however, does not state very clearly his own opinion on the nature of this growth. But from his description it may be inferred that he regards it as a membranous growth or production from the conjunctiva. Perhaps the difference between this and the view of Professor Scarpa is more imaginary than real.

The operation is minutely and accurately described. The sum of it may be said to be, that the growth must be removed by the knife; and if fungous or unhealthy granulations succeed, the scissors, with the occasional use of blue vitriol or lunar caustic, are to be employed. When the pterygium assumes a cancerous aspect, removal of the contiguous part of the eye may be requisite; and if farther advanced, extirpation of the contents of the orbit may be the only remedy. On the observations on the removal of extraneous substances from the eye, and on injuries of the eyeball, though judicious and instructive, it is unnecessary to dwell.

In the next chapter, on tumours formed within the orbit, and protrusion of the eyeball, the reader finds much interesting matter. It would, however, have been better had Mr Guthrie altogether separated suppuration of the bottom of the orbit, from encysted and other tumours occurring in that situation. The first not only differs in nature, but demands a different line of treatment entirely. In the former case, simple incision or puncture may be sufficient to effect a cure. In the latter, extirpation generally in the case of encysted tumours, and always in the case of sarcomatous or malignant growths, becomes a necessary measure.

This proceeding, though attended with no alarming hemorrhage, is by no means free from danger. The necessary incisions may be followed, Mr Guthrie has shown, with severe erysipelatous inflammation; or, if the tumour is attached to the periosteum of the orbit, with inflammation, spreading to the dura mater, cerebral membranes, and the brain, so as to prove fatal in a very short space of time. Of the latter the author quotes a good example from Langenbeck. It is the duty of the surgeon in such circumstances to watch his patient with the most assiduous vigilance and circumspection, and upon the first appearance of inflammatory symptoms affecting the brain to put the antiphlogistic treatment in prompt and energetic employment.

To tumours behind the periosteum, whether issuing from the bone, or connected with the sphenoidal or ethmoidal sinuses, or proceeding from the dura mater or the brain, the same considerations are applicable.

In each of these cases, in aneurisms of any of the arteries within the orbit, and in tumours, hydatids, or enlargements of the lacrymal gland, more or less protrusion of the eyeball takes place, constituting the affection known under the general denomination of *exophthalmos.*

Among tumours of the orbit affections of the lacrymal gland hold a conspicuous place. The alleged frequency of these disorders, as maintained by Mr Todd, is doubted by Mr Guthrie, who states, that, among nearly 12,000 patients who have attended the Eye Infirmary to which he is attached, he has met one case only of suppuration, and none of scirrhus. In proof of the accuracy of the statement regarding the latter disease he further adduces the testimony of Beer, Schmidt, Desault, and Boyer, according to whose observations scirrhus of the lacrymal gland is never seen primarily, and is met with only as a consequence of cancer of the eye. Richerand quotes a single case in which Guerin extirpated the gland, and which he is disposed to regard as the only example of isolated scirrhus, or independent of cancer of the orbit. Warner, on the contrary, several times saw it so changed in structure as to require extirpation. Demours states that he has seen several cases of it. Travers removed the gland entirely in a state of true scirrhus; and Todd has more than once performed the same operation. These facts show that it is difficult to draw any general conclusions regarding the comparative frequency of primary cancer of the lacrymal gland. The remedy is extirpation, the manner of performing which is given in the cases of Mr Todd and Mr O'Beirne.

Of *Staphyloma* Mr Guthrie admits two forms. The first, however, is rather to be viewed as an instance of extreme thickening, with some opacity of the cornea, which is more spheroidal and prominent than natural, but through which the iris can still be distinguished. It is not at all wonderful that this form of morbid change is quite irremediable. The second is the ordinary form of staphyloma described by Richter, Scarpa, Beer, Schmidt, and all the writers on ophthalmic surgery.

The anatomical characters of this disease, which is the consequence of inflammation, have been represented by most of the foreign oculists to consist in swelling or thickening of the cornea, outside and inside, with swelling and protrusion of the iris, which is made to adhere to the inner surface of the cornea. This is matter of fact, which cannot be denied. But they have imagined another circumstance of a more doubtful nature, the reality of which Mr Guthrie justly questions. We allude to the idea, that the secretion of the aqueous humour in the posterior chamber proceeds, while the cornea yields to pressure, is distended, and becomes staphylomatous. It is not difficult to show that this is assuming an event posterior in its existence, to account for another already established. In point of fact, the staphylomatous protrusion of the cornea has taken place previous to any unusual accumulation of aqueous humour; and Mr Guthrie has been at some pains to show, that whatever accumulation takes place is by no means sufficient to produce the inordinate swelling of the cornea.

Staphyloma has been observed to be frequent in infants and children, more especially after the pustular inflammation which not unfrequently attacks the cornea during or towards the termination of confluent small pox. Scarpa attempted to show that this prevalence of staphyloma in young subjects depends on the pulpy and succulent texture of the cornea at that period of life. A more probable explanation of this fact is derived by Mr Guthrie, from the proneness which variolous inflammation attacking the cornea manifests to produce ulceration of that membrane. The hypothesis of Scarpa is further invalidated by the fact, that staphyloma may succeed every inflammation of the eye in which the cornea is severely affected through its substance. Thus purulent ophthalmia, when epidemic in the army, afforded a large proportion of cases of staphyloma; and Mr Guthrie refers this event only to the sloughing and ulceration which that disease often produced. We have seen genuine staphyloma take place in consequence of acute inflammation of the eye ascribed to the presence of a minute quantity of quicklime. Does the sulphuric acid when maliciously thrown into the eye produce the same result?

In every case of genuine staphyloma the eye is rendered utterly useless as an organ of vision; and the duty of the surgeon is confined to the means of affording relief from the irritation resulting from the inordinate projection, and occasional mechanical inequality of the anterior surface of the eye. The precise object of the surgeon in affording this relief has never been very distinctly stated by authors; and the student is hence told in general terms, that the operations for staphyloma are effected either by caustic or by the knife. Even the present author has not attempted to refer his curative means to one or two general principles; and the reader is allowed to collect the results from a variety of statements not altogether naturally connected.

Richter alone, in practising the use of escharotics to produce a small ulcer, avowed it to be his purpose to *evacuate the thick tenacious* humour, which is the immediate cause of the opacity and unnatural tumefaction of the cornea. The benefit of this method, after repeated trials, is denied by Scarpa, who is therefore charged by Mr Guthrie with not understanding the intention of Richter, and the manner of its application. Mr Guthrie might have justly taken the present opportunity of stating succinctly the theory of the operation.

In all cases of staphyloma the objects to be held in view are the restoration of the natural shape and spherical figure of the cornea; and in most cases this is to be effected by removing irregularity, and diminishing undue prominence. The first of these objects may be effected by caustic or by the knife, used in such a manner as remove successively any point more projecting than another. The second is to be done by evacuating such fluid, whether it be part of the aqueous humour degenerated into an opaque fluid, or a new secretion, as contributes with the thickened cornea to inordinate convexity and prominence of the eyeball. With this view any of the potential caustics may be employed, as Richter recommended, to produce a slough, which, when thrown off, leaves an opening, through which the morbid fluid trickles, till its entire discharge allows the cornea to subside. When the knife is employed, in consequence of the proximity of the iris to the cornea, not only the crystalline lens, but the vitreous humour may escape. But as the eye, notwithstanding the assertion of Demours, is invariably useless, this is injurious in no other mode than as inducing too great diminution of the eyeball.

From *extirpation* we pass to the lectures on cataract, its varieties, and the several modes of operation.

These lectures are numerous, elaborate, and minute; and as we cannot hope to give any thing like a comprehensive view of the subjects treated, and the questions discussed by the author, we shall merely direct the attention of the reader to the subject in a very general manner.

It is well known that the anatomical character of cataract consists in more or less opacity of the crystalline lens, or its containing capsule, or of both. The causes which tend to produce this diminution of transparency are by no means satisfactorily known; and surgeons have been contented with enumerating all the circumstances under which cataract has at any time been observed to take place. An opinion very generally prevalent of late years is, that cataract or opacity of the lens is the effect of inflammation, especially of the capsule; and this is doubtless observed to take place in syphilitic or mercurial inflammation of the iris, and other internal tissues of the eye. Professor Walther especially has remarked a wreath of vessels at the pupillary edge of the iris sending vessels to the centre of the anterior layer of the capsule, and a network of vessels issuing from the posterior layer into the substance of the lens,—both of which, he conceives, are morbid, and constitute the agents of that alteration in its structure which impairs its transparency. Mr Guthrie, while he admits that this appearance is that of inflammation of the lens, and that it may produce one species of cataract, denies, however, that it can account for all. It is, he justly remarks, inflammation propagated from the contiguous tissues to the capsule, and doubtless diminishing the transparency of the lens, but forming not a true but false cataract. In some instances even the lens is not affected; and when the inflammation has subsided, the white, gray, or brown appearance of the capsule disappears entirely, and the parts become again diaphanous.

He takes the same view of the theory lately advanced by M. Gondret regarding the inflammatory origin of lenticular or capsular opacity, and in short of all those instances of cataractoid blindness coming on suddenly and from obvious causes. "If inflammation," he contends, "be the cause of pure lenticular or capsular cataract, it is an inflammation *sui generis*, depending on the peculiar structure or organization of the parts, and the progress of which is not likely to be better known to us, until this peculiar structure or organization shall have been more fully investigated." An additional reason might have been added. Whatever be the structure of the capsule, it is almost certain that the lens presents no traces of organization; and it is incompatible with the known laws of pathology to admit the existence of morbid action in parts which are not proved to be organized.

In his classification of the several forms under which cataract takes place, Mr Guthrie adopts the ordinary mode of dividing the true or genuine, according as they take place in the lens, in the capsule, or in both at once. In contradistinction to these he regards all the instances of opacity in the lens, its capsule, or both at once, which succeed inflammation, as examples of false or spurious cataract.

The species of lenticular cataract are distinguished into hard, fluid, soft, and caseous. This division, however, in its further application, is singularly deficient in scientific precision and clearness. Thus, though Mr Guthrie informs his reader that the lenticular cataract consists of the four species, or rather sub-species, above-mentioned, it turns out, according to the description of Mr G. himself, that the only instance almost of lenticular cataract is hard,—that the soft or caseous form belongs chiefly to capsular cataract,—and that, though the lens on becoming opaque may acquire a preternatural fluidity, yet the fluid cataract is very frequently, if not generally, found either in the *humor Morgagni*, or in the capsule itself. This want of precision is so much the more to be regretted, that the detailed descriptions of the several varieties are minute, and bear every mark of great accuracy.

It would be unreasonable, with our limited observation, to attempt any correction of the results given by an oculist so experienced as Mr Guthrie. But, so far as we have been enabled to compare cases of cataract with the distinctions laid down by authors, we think much confusion and some imagination have all along prevailed in marking the relative consistence of different species and varieties of cataract. Cataract depending on opaque lens is certainly often pretty firm, as represented by Mr Guthrie. But, notwithstanding the elaborate manner in which he has attempted to distinguish this hard variety of lenticular cataract, we are convinced, from the observation of a considerable number of cases operated on by different surgeons, that it is next to impossible to say, before the needle is introduced into its substance, whether it be hard or soft, fluid or caseous.

The varieties of capsular cataract are much more frequently hard, or at least firm, than those of the lenticular disease; and in general it may be regarded as pretty uniform, that in most cases of capsular cataract, if the lens is in any degree opaque, its consistence is fluid, soft, or caseous. In the latter case it corresponds to the capsulo-lenticular cataract of the present author.

The different modes of operating for the cure of cataract are sufficiently well known. Though various surgeons, and especially Mr Ware, and more recently M. Gondret, conceived the practicability of restoring the transparency of the eye by various applications without operation, this idea has never gained any firm hold of professional opinion; and the proposal may safely be pronounced as utterly inapplicable to genuine cataract. Mr Guthrie has given this subject some attention; and though he infers that it is proper to confirm the opinion, that opacities of the capsule may sometimes be arrested, frequently diminished, and occasionally removed, by due appropriation of means, yet this is chiefly applicable to such opacities as are the consequence of common inflammation, or to the true idiopathic cataract in an incipient state. He doubts entirely the possibility of removing a well-formed opacity of the lens either by external applications or by internal remedies.

By the operation for cataract is meant the removal of the opaque lens and capsule from the axis of vision; and this may be accomplished in two modes, either by direct and entire extraction, or by destroying their connection with their nutrient parts, when they undergo a species of solution, either in the vitreous or in the aqueous humour. According to the mode in which the latter form of operation is effected, it is termed couching, depression, reclination, (Willberg and Beer;) or division, or, in general, displacement, (Guthrie;) and according to the mode in which the instrument is introduced for effecting these purposes, before or behind the iris, it is denominated the posterior or the anterior operation, (*keratonyxis*, Conradi, Jaeger, and Buckhorn) κεϱατονυξις, *corneæ punctio.*) In either of these cases it is manifest that the eye is deprived of a part, important for its great power in refracting and concentrating the luminous rays; and consequently, that, as its place is occupied by aqueous humour only, vision can no longer be the distinct and perfect process which it was in the original condition of the organ. This conclusion, derived from optical principles, accurately corresponds with the state of eyes which have been deprived of the crystalline lens in the operation for cataract. The vision is indistinct, in consequence of the low and irregular refracting power of the medium which is substituted for the lens; and this defect can only be remedied by the use of an artificial lens in the form of a convex glass, which supplies the deficiency of concentrating power. It is hence necessary to explain to patients, that the operation for cataract, though requisite as the only means of removing an opaque body, the presence of which forms an impassable barrier to the rays of light, cannot, nevertheless, restore the degree of vision originally possessed by the sound organ.

Much has been said by different oculists on the proper period for operating, on the propriety of operating when only one eye is affected, and in the case of both eyes, on the propriety of operating on both at the same time. These several topics Mr Guthrie discusses with much deliberation and acuteness, and is at some pains to explain the principles by which the oculist is to regulate his proceedings. He directs the attention to the circumstances in the general constitution of the eye and its textures, which favour and contraindicate operation.

In opposition to the opinion of Samuel Cooper and Mr Travers, that the objections against operating in the case of a single cataractous eye, the other being sound, he scruples not to say, that, as far as the improvement of sight is concerned, the operation ought not to be attempted on one eye while the other remains sound. He admits, that, if the sight of the sound one becomes deranged from the commencement of cataract or any other cause, the objection is completely obviated. In local or accidental cataract he does not believe that the presence of an opaque lens in one eye exercises any influence on the other; nor would he trust to its removal for preventing the formation of opacity in the other at any subsequent period. The only reason which he admits for justifying an operation under such circumstances, is to remove deformity, and restore to the countenance that intelligence which is much diminished by the presence of the opaque lens.

The opinion that the removal of cataract from one eye will prevent the formation, or cause the disappearance if forming, of opacity in the lens of the other, has been lately revived by

Mr Stevenson. This point has been considered with much attention, and illustrated with a good deal of curious matter by Mr Guthrie, who thinks, on the whole, that there is some ground for that part of the assertion of Mr Stevenson, that "the removal of cataract from one eye may cause the disappearance of an incipient opacity in the other." In four cases out of a very great number he has seen the removal of an opaque lens from one eye succeeded by the disappearance of an incipient opacity, probably of soft consistence, and the consequence of capsular opacity in the other. But he has not had occasion to remark the disappearance of a well-formed opacity of the lens, evidently hard, in consequence of operation on the other eye; and he believes, therefore, that no such result takes place when the lens alone is opaque, and especially where it is indurated. On the contrary, he selects from a great number of cases, four in which the operation was highly successful in all respects, in which, nevertheless, the completion of cataract in the opposite eye was not prevented.

In the case of opacity of both eyes Mr Guthrie is decidedly hostile to the practice of operating on both at once as a general rule. The exceptions he derives, as Beer does, from those circumstances, either necessary or accidental, which may render success doubtful; but the final result may be understood from the following avowal:—"If I were the subject of this disease myself, I should choose to have one eye operated on first, and the second done at a subsequent period." When cataract is completely formed in one eye long before it impedes vision in the other, he advises the patient to submit to his disease, keeping his health in good condition, until the eye last affected become nearly unserviceable, when the operation might be conveniently performed on that first affected.

The different modes of operating practised by different oculists are described most minutely and accurately; but for these details we refer to the volume itself. We shall simply notice one or two points relative to the comparative merits of the several modes of operating.

In all cases of operation it is requisite previously to dilate the pupil by means of hyoscyamus or belladonna. It is singular that Dr Bowen of Naples claims, in his Treatise (published in 1823) on *Hyalonyxis*, or the operation by piercing the vitreous humour, the merit of recommending this practice. In preferring this claim, he very unguardedly exposes himself to the charge of complete ignorance of the practice of the majority at least of oculists in this country. Mr Guthrie states, that, in the first edition of the present work published in 1823, he recommended the use of belladonna for dilating the pupil, previously to any operations within the eye, as a practice which he had followed for years, and which he did not conceive to be anything new. We can state positively that in 1816 and 1817 the practice of dilating the pupil by means of belladonna was in constant and established use in the hospital of this place in every operation to be performed for cataract, whether by depression or extraction. There is no doubt, from the testimony of Conradi, Langenbeck, and others, that it was in use in Germany long before this period.

Extraction, which alone is entitled to the character of a radical cure, though competent to all, is indispensable only in cases of cataract depending on induration of the lens. It is, nevertheless, neither expedient nor practicable in all cases of hard lens, since there are many circumstances in the eye or its appendages, or in the constitution of the patient, which may render it uncertain, if not quite inadmissible. Thus, to the success of its performance it is indispensable that the eyelids open well, be free from inflammation or any of its consequences, as œdema, granulation, and disease of the Meibomian glands; that the lachrymal passages be healthy, and the eyelashes straight; that the eye itself be neither too much sunk in the head nor too prominent; that the cornea be transparent, sound, and free from inflammation; that the iris be plane, its pupillary margin free from adhesions, and the anterior chamber of its ordinary dimensions; that the pupil be steady and regular in its motions, yet not too dilatable; that there be neither glaucoma nor amaurosis; and, lastly, that the patient be free from pectoral disease, gout, rheumatism, or other disordered states, which, by impairing the vigour of the constitution, may predispose to unhealthy inflammation or other unfavourable results.

The principal evils against which the operator should be on his guard in this operation, are injury of the iris, laying the foundation for protrusion of that membrane, its implication in the wound, and irregular or even closed pupil; expulsion of the vitreous humour; and suppuration of the eye. To obviate the first set of evils the choice of a proper eye, caution in operating, and care in the after-treatment, are generally sufficient. The second accident is injurious rather from the inflammation which succeeds, by the glutinous matter of the vitreous humour adhering to the incision, than from the actual loss of that humour. According to Mr Guthrie, if one-fourth of the vitreous humour be lost, the sight is scarcely impaired; if the loss amount to one-half vision will be imperfect, and the result may be doubtful; but if a larger quantity escape, the collapse of the eye may be fatal to the organ, (p. 335.) It is best obviated by placing the patient immediately in the horizontal position. The third accident may occur under any circumstances, but is most likely in unsound constitutions. It can be resisted in no other mode than by prompt and energetic antiphlogistic treatment.

Lastly, against extraction it may be urged, that, if it fail in the first attempt, it is not capable of being performed a second time. The operation of displacement again, comprehending depression and reclination, may be performed in all cases in which extraction is practicable, and in several in which it is inadmissible. It is applicable to all cases of hard lenticular cataract in which operation is advisable. It may be performed when the eye is so deep in the orbit as to render extraction difficult; when the eyelids are contracted or diseased; when the lachrymal passages are affected; when the cornea is partially opaque, or inflamed, or flattened; when the dilatation of the pupil is irregular; when the iris is vibratory; when, in the case of operation being otherwise desirable, doubts are entertained as to the sound state of the eye; when the motions of the organ are unsteady or spasmodic; and, lastly, when the health of the patient is doubtful.

Against this operation, however, various objections have been urged by Wenzel and the other partizans of the method of extraction. These objections Mr Guthrie has combated with great sense and force of reasoning, and we think with perfect success. He has in truth placed the relative merits of this operation in the clearest light; and whoever still entertains doubts of the propriety of the method by displacement should peruse and carefully consider what he has said on this subject in the present work.

The only serious objection is that of the rising of the lens, sometimes perhaps years after depression. On the supposed causes of this, and especially the influence of the vitreous humour on re-elevating the lens, Mr Guthrie makes some judicious observations. He shows that the arrangement of the vitreous humour in the cells of the hyaloid membrane prevents the lens from passing through any part of this humour, except that by which it has been depressed, so that the latter rises nearly as it has been pushed down, but does not occupy exactly the same situation.

Reclination through the cornea, (*keratonyxis,*) Mr Guthrie regards as possessing no advantage over that through the sclerotic. He admits, however, that between the former and the old operation of depression there is no comparison. The operation by dividing the capsule and lens is applicable to cases of soft cataract only. When *cut up*, as it is termed, in this manner, the fragments are supposed to be absorbed, or rather dissolved in the aqueous humour. The objections against this operation are of much the same kind as those against the others; and are to be obviated by the judicious adaptation of the method to the case under treatment. According to the testimony of Mr Guthrie himself, vision is as perfect after this as after extraction. He nevertheless avows his preference of the posterior operation of Scarpa, even in cases of large fluid capsulo-lenticular cataracts.

Congenital cataract differs somewhat in its anatomical characters from the ordinary cataract of adults. Professor Walther supposes that it is not formed in consequence of disease; but that, as the lens is naturally turbid in the fœtus, it continues so in consequence of some check given to the progressive developement of the embryon at some stage of its growth. To this idea it may easily be objected that it is difficult to conceive any cause which could arrest any natural process in the eye, without producing an analogous interruption in all the other processes in the life of the fœtus. Mr Guthrie admits, nevertheless, that this idea receives some countenance from the fact remarked by him, that the lens and capsule in one species of congenital cataracts in which the capsule is thick and striated, and the lens soft and transparent, never increase in size in the same proportion with the rest of the eye; and when the pupil is well dilated the small opaque lens may be seen well-defined on the vitreous humour. In such circumstances a sudden check appears to have been given to the growth of the lens at a certain period of the growth of the ovum.

Schmidt, Beer, and others of the German oculists, again, are disposed to believe that many of the cases of congenital cataract, supposed to exist previous to birth, are in truth the consequence of rupture of the capsule during the violent convulsions which occasionally occur shortly after birth. This supposition arose probably from the circumstance that congenital cataracts are often capsular only, the lens having disappeared by absorption. But it is justly remarked by Mr Guthrie, that, in admitting convulsive rupture of the capsule as a cause, it must not be forgotten, that few children who have suffered from convulsions have cataract, and that, conversely, cataracts are observed in many children who have never at any time been affected with convulsions.

The opinion of Gibson, and the ingenious hypothesis of Cunningham Saunders on the congenital cataract, are well known to all those who take any interest in ophthalmic surgery. The first denied the fact of absence of the lens, and contended, that, though simple membranous cataract is not uncommon eight or ten years after birth, it is not found in the infant at an earlier period. These cataracts, he believed were at that time of the milky kind, and, the fluid having been absorbed, the opaque capsule which originally contained it, formed the membranous cataract found at a later period of life. Saunders conceived that the lens became first opaque, was forthwith removed by absorption, while the anterior plate of the capsule, retiring on the posterior, formed a hard opaque membranous body in the site of the lens. Mr Guthrie takes pains to reconcile these two opinions, which in truth differ much less in reality than either of their ingenious patrons themselves imagined.

To Mr Saunders belongs the merit of demonstrating the general practicability of operating in early infancy. The peculiarity of his method consists, it is well known, in cutting out, as it were, the central portion of the opaque body, generally by the anterior, sometimes by a posterior operation. The experience of Mr Guthrie on this subject, however, leads him to prefer the posterior operation.

The chapter on the operation for artificial pupil, though a little shorter, is not less elaborate. After distinguishing the operation into four successful methods, division, (*coretomia, iridotomia*) excision, (*corectomia, iridectomia*) separation, (*coredialysis, iridodialysis,*) and the union of separation and excision, Mr Guthrie traces the history of this operation from the days of Cheselden, Morand, Janin, and Sharp, down to those of Scarpa, Gibson, Maunoir, Beer, Buckhorn, Langenbeck, Quadri, &c.

The object of the operation is easily understood. In cases of adhesion, or closure of the pupillary margin of the iris, while the cornea is transparent, and the lens and vitreous humour are presumed to be so, it becomes requisite to make an aperture in the centre of the closed pupil, to admit the rays of light to the humours and retina. As it is impossible to give such an aperture the motions possessed by the original pupil, it is of great moment to accommodate its size to the average extent of the pupil in a moderate light. Though it is rarely practicable to give this aperture the circular form, it is always important to make it approach as near as possible to that of the original pupil.

When an aperture cannot be made in the centre of the iris, the other parts of the membrane are eligible in the following order; 1*st*, The lower part of the iris inclining inwards; 2*d*, The internal a little below the tranverse diameter of the eye; and 3*d*, The inferior and external; the upper, the corresponding part of which is covered by the eyelid, being the least eligible. Mr Guthrie justly prefers the lower parts of the iris, as those through which the line of vision is least incurvated, and therefore less likely to produce squinting. As the cornea, in some cases requiring an artificial pupil, is partially opaque, the situation of the aperture should always be fixed in relation to that which is clear.

After a distinct enumeration of the different states of the eye requiring the formation of an artificial pupil, Mr Guthrie proceeds to consider the merits of the different modes of effecting it; and while with much judgment he refuses to concede absolute superiority to any one over another, he nevertheless shows that there are circumstances in the different states of the eye requiring operation, which render one mode of operating more applicable and expedient than another.

It would be foreign to the purpose of this sketch to enter into the details of the lengthened inquiry before us. We can assure the student that his labour will be well repaid by a clear and instructive critical view of the proposals and practices of different oculists and surgeons.

The present article, though not short, exhibits little more than a passing glance at the various subjects discussed in the volume of Mr Guthrie. It has been already said more than once, that these subjects are investigated in the most deliberate and comprehensive manner. We repeat here what has been said at the outset, that Mr Guthrie has not confined himself to the results of personal experience, however sound that may be, and however extended the opportunities for acquiring it. With much research he has collected information from all the best and most respectable authorities, and with a spirit of liberal and judicious criticism he has endeavoured to explain to the student the comparative merits of the methods proposed by different operators.

In conclusion, it need only be said that the work of Mr Guthrie exhibits the most comprehensive view of the present state of knowledge on the operative department of ophthalmic surgery, and communicates in general sound notions on the pathological principles by which operations ought invariably to be regulated. It is, in short, a work which the student of ophthalmic surgery will find it his interest to study attentively and deliberately, and without which no practical oculist can conscientiously exercise his art.

ART. II.—1. *Outlines of Human Physiology.* By HERBERT MAYO, Surgeon and Lecturer on Anatomy. London, 1827. 8vo. pp. 406.

2. *Laws of Physiology, translated from the Italian of B. Mojon, Professor Emeritus of the University of Genoa.* By GEORGE R. SKENE, Member of the Royal College of Surgeons, London, &c. London, 1827. pp. 126, 8vo.

IT is correctly observed by Mr Mayo, that a system of physiology or a detailed treatise fitting the magnitude of the subject, would little suit the present thriving state of medicine. The labour necessary to compile such a work would meet with but an inadequate return during the few years of popularity it might enjoy, before it was rendered obsolete by the rapid strides which the science is now daily making. The kind of work which the present state of physiology seems best to admit of, and which at the same time is peculiarly called for to enable the student to discern the truth amidst the numerous controversies which exist throughout all its departments, is a succinct elementary treatise, comprising an account of the chief facts which have been substantially established relative to the bodily functions,—and separating them as much as possible, on the one hand, from the laborious experimental inductions and tedious controversial discussions, through which a knowledge of them has been at length attained to, and on the other, from the mass of hypothetical and uncertain statements with which, even by the most recent physiologists, they have been too frequently associated. Such appears to have been the object held in view by Mr Mayo and by Dr Mojon and his translator.

The "Laws of Physiology" of Dr Mojon we have been induced to notice chiefly by the high encomiums bestowed on the treatise by the Italian editor, and by the English translator, Mr Skene. But we do not think it calculated to fill up the gap in this department of English medical literature. The work consists of a series of 695 aphorisms, intended to comprise all the established general facts in physiology. A series of aphorisms does not appear a very appropriate mode of unfolding the truths of a science circumstanced as physiology now stands. It is at best an unattractive method of enunciation, in which abstract facts present themselves to the understanding, divested of their most interesting feature,—their mutual relations. But in the instance of physiology it is peculiarly inconvenient, because the known, the doubtful, and the unknown are too apt to be enunciated together, with the same apparent character of authenticity.

Neither does it seem that Mr Skene has approached his subject with the caution required of him, considering the method in which he had resolved to handle it. For a philosopher in search of "axioms," he displays rather an undue spirit of enterprize and innovation. Thus, in his preface he announces his opinion,—that the lacteals are intended not to convey chyle from the intestines into the circulating system, but to carry a mucous fluid secreted in the mesenteric glands for lubricating the intestines,—that the function of the lymphatic glands is not to effect changes on the lymph preparatory to its being discharged into the blood, but to secrete the fluid of perspiration,—that the uterus is habitually in a state of contraction, the object of which is to promote the circulation of the blood in the veins,—that in the act of parturition the uterus is not thrown into a state of violent contraction, as all physiologists and accoucheurs before him have believed, but that, like the stomach in the act of vomiting, its contractions and contractility are suspended, and it becomes an inactive membranous sac, discharging its contents under the influence only of the pressure caused by the contractions of the diaphragm and abdominal muscles.

Statements like these, advanced without any commensurate attempts to support them by the evidence of facts, and in the face of experimental results, numerous, concordant, and generally admitted as true,—will naturally breed a distrust of the author's power to lay down a series of physiological laws, with a cool and unbiassed judgment. Accordingly, many of his aphorisms will, we apprehend, be found erroneous. Take for instance the following. " The force and fire of the imagination are apparent *after much eating and drinking*."—" Man employs but one eye when he wishes to see an object very distinctly."—" The ribs are the only bones in the body whose motion begins with birth, and *ends with death*."—" The gastric juice is a composition of much water, a little gelatino-mucous matter, muriate of ammonia, muriate of soda, and phosphate of lime."—" Coughing is executed by making repeated rapid and alternate movements of inspiration and expiration, accompanied by the action of the diaphragm and abdominal muscles."—" Saliva is *greedy* of oxygen."—" In the adult man animal heat is habitually *from a hundred and four to a hundred and ten* of Fahrenheit's thermometer."—" Circular fibres of muscles, without a fixed point of attachment, are all incapable of voluntary contraction and dilatation." We fear these extracts betoken a spirit of hasty and careless generalization, quite incompatible with the nature of the task which the author had laid down for himself.

Let us now turn the reader's attention to a work of a very different stamp, the " Outlines of Physiology of Mr Mayo." This is perhaps the best manual of physiology that has hitherto been published in Britain. The author has selected with great judgment and impartiality the most accredited and important facts and doctrines of the science. While, in relating them, he has stripped away all superfluous details, with which most of them have been encumbered in the course of frequent controversy, he has at the same time been eminently successful in leaving them supported and illustrated by the necessary experimental evidence. His description is uniformly perspicuous, as well as concise: The most abstruse departments of the subject are unfolded with such simplicity, that we should think no reader, whether professional or unprofessional, can fail to be deeply interested in his narrative. It is no small additional praise, and a great recommendation to those for whom chiefly the treatise is intended, that his language is always pure and classical, in a degree seldom witnessed in the medical writings of the present day.

Of a work so terse and elementary it would of course be fruitless to attempt to convey an adequate impression in the limited space we can afford. But we shall attempt to do the author all the justice in our power, by noticing briefly his plan of arrangement, and by exemplifying the general spirit of the treatise by an analysis of one or two of the principal chapters.

After an introductory disquisition on the tendency of physiological studies in reference to morality and religion, on the physical and chemical properties of organized matter, and on the vital phenomena and properties,—he proceeds to consider the several functions of the body in fifteen chapters. He *first* treats of the theory of the circulation, and the composition and properties of the blood;—*next*, of muscular action, the intimate structure of muscles, the distribution of the nervous filaments in them, their tonicity, and their contractile power;—*thirdly*, of the forces which circulate the blood, including an account of the mechanism and contents of the chest, of the influence of the resilience of the lungs and the suction power of inspiration on the progress of the blood in the veins, of the action of the heart, the nature of the pulse, the motion of the blood in the capillaries, and the peculiarities in the cerebral circulation;—*fourthly*, of the pulmonary circulation, the structure and capacity of the lungs, the changes caused by respiration on the air and on various gases, the influence of the phrenic and pneumo-gastric nerves on respiration;—*fifthly*, of the circulation through the body, and under this head, of fainting, of transfusion of blood, of asphyxia, of the effects of introducing various foreign substances into the blood, of secretion and nutrition, and the powers that influence them, of the sources of animal heat, and the proofs of venous absorption. The next chapter, which treats of digestion, is divided into six sections, the first comprising some observations on hunger and thirst, mastication and deglutition; the second an anatomical description of the alimentary canal and collateral organs; the third and fourth, an account of the formation of chyme and chyle; the fifth, a detail of the functions of the great intestine; and the sixth, an enumeration of the various articles used as food. From this subject he passes to the consideration of the lacteals and lymphatics, the mesenteric and lymphatic glands, the chyle and the lymph; and then, as accessory to the function of digestion, he considers, on the one hand the urinary organs, and the urine viewed as an excretion eliminating superfluous azote and foreign substances accidentally absorbed,—and on the other hand the skin, its power of absorption, its secretions, the standard heat of the body, the phenomena of hybernation, and the effects of extreme heat and cold. The two subsequent chapters consist of an elaborate disquisition on the anatomy and functions of the nervous system, and on the organs of the senses. On these topics he has entered into greater details than on any other. After some preliminary observations on the nature of sensation, perception, instinct, and volition, and a brief anatomical description of the brain, spine, and nerves, he attempts to collate the numerous experimental researches lately made on the functions of these organs and their component parts, and to settle what parts are necessary to sensation, instinct, and volition, and what knowledge has been hitherto thrown on the functions of the cerebrum cerebellum, and medulla oblongata; and he concludes the chapter with an analysis of the phenomena of sleep, dreaming, and sensorial illusions. It is unnecessary to follow him through the particulars of the succeeding chapter on the five senses. His description is uniformly clear, and his investigation of the numerous disputed points relative to the functions of the organs of sense intelligible and impartial. The four subsequent chapters are occupied with an inquiry into the mechanism of the human voice,—the attitudes and movements of man,—the physiology of generation, and the minute changes that take place in the growth of the different textures of the body.

Having thus given a general view of the plan and contents of the work, we shall now exemplify Mr Mayo's method of treating his subject by an analysis of one or two of the chapters; and for this purpose we shall select that on the circulation of the blood through the body, and part of that on the functions of the brain, spine, and nerves.

The chapter on the circulation through the body is perhaps the most finished in the book. He commences with stating, that the leading object of the circulation is the transmission of arterial blood through the capillaries, and the leading phenomena it gives rise to are *excitement*, *secretion*, *distribution of heat*, and *absorption*.—1. The propulsion of arterial blood through the capillaries is indispensable for maintaining that excitement of the nervous and muscular systems which is necessary for the continuance of their functions. For *first*, if the impulse of the blood is diminished by hemorrhage, mental impressions or otherwise, the nervous system becomes at once torpid, and the muscles relaxed; and when these effects originate in hemorrhage, they are removed by transfusion of blood into the vessels:—

Secondly, the imperfect arterialization of the blood produced by a deficient supply of air to the lungs is attended with similar consequences:—*Thirdly*, an unusual rapid oxygenation of the blood, as by breathing oxygen or nitrous oxide gas, causes increased excitement:—*Fourthly*, the substitution of water for blood in the vessels enfeebles excessively the muscles and sensibility of the nerves:—And *fifthly*, the introduction even of air and of oily matters, and many medicinal substances, causes more or less disorder in the system, and frequently death, in consequence of their obstructing the capillaries or poisoning the tissues where they ramify.—2. Arterial blood is the material from which the secretions are formed. The secretions are of two classes, the one comprising the *nutritive secretions*, or those belonging to the constitution of the organs where they are found, the other the *functional secretions*, or those whose separation is necessary to the function of the organ which secretes them, or to adjoining organs, but which are mostly mechanically removed. For the former species no special organization seems requisite. Although under the influence of the nervous system, nutritive secretion does not entirely depend on it, for it exists in plants which have not any nervous system, and in animals it continues in a part after its connexions with the brain and nerves are destroyed. It is much under the influence of physical impressions, exercise, pressure, and the like; but it is affected but by a few medicines only, such as mercury, iodine, and perhaps potass. For the latter or functional species of secretions a considerable diversity of organization may be found in the body—a uniform vascular surface as in the skin,—a similar surface accumulated in folds, as in the mucous membrane of the intestinal canal,—and fleshy-like particles, as in the liver and kidney, which, however, in their ultimate or minute organization, may be considered as conglomerated membranous surfaces. Functional secretion is very much under the influence of the nervous system, and of medicinal substances introduced into the body. The precise place where secretions of this class are formed, whether it is in the general mass of the blood, or in the capillaries of each secreting organ, still remains to be determined.—3. The distribution of animal heat is to a certain degree dependent on the impulsion of arterial blood through the capillaries, and its conversion into venous blood during its passage through them. This is shown by the fact, that venous blood has a less capacity for caloric than arterial blood, and by the close ratio that subsists between the degree of animal temperature and the perfection of the respiratory system. The whole amount of the animal temperature, however, is not accounted for in this way, and part is probably owing to nervous operations, for Mr Brodie found that a decapitated animal, left undisturbed, did not cool more rapidly than another in which respiration was maintained artificially.—4. Absorption seems to depend on the circulation alone, the various substances which are absorbed being conveyed into the vessels by simple imbibition. The doctrine of simple imbibition has been espoused unreservedly by Mr Mayo, and the evidence in support of it is very powerfully summed up. It consists in the passage of coloured fluids through serous membranes in the living body,—the poisoning of animals by deleterious substances placed in contact with the outer surface of veins, and the effect of pressure, and still more of suction in preventing or suspending the action of poisons. The proofs that the veins are the channel through which substances are absorbed are also neatly and satisfactorily stated: It is impossible to abridge them within moderate compass; and besides they are now too familiar to require repetition here.

The author has bestowed much pains on the chapter on the Anatomy and Functions of the Nervous System, and has treated that intricate and obscure subject in a very masterly manner. We shall give a short analysis of those sections of it, in which he attempts to settle what are the results of the late experimental inquiries into the functions of the several parts of the brain and spine.

In this summary he throws aside all hypothesis and conjectural results, of which many may be found interspersed throughout the inquiries alluded to, and details only those which are established by the concurring testimony of every experimentalist. They are the following:—1. Sensation, volition, and the commoner instincts, appear to depend on the nerves, spine, and medulla oblongata. For they exist in animals at the bottom of the scale of creation, that have no brain or cerebellum; they have been likewise found to exist in those acephalous infants, whose nervous system was confined to nerves, a spinal chord, and a small bulb equivalent to the medulla oblongata; and, if the brain, cerebellum, and optic tubercles be removed in a living animal, it retains its senses, with the exception of sight, and likewise preserves the action of the voluntary muscles, somewhat impaired however, and irregular.—2. It farther appears, that each portion of the body has a corresponding part of the nervous system appropriated to it. For when the nervous system is divided into several segments, impressions made on parts supplied with nerves from the several segments are followed by corresponding muscular actions.—3. The great end therefore served by the continuity of nervous tissue in the spine and brain, is to connect reciprocally the impressions transmitted to or originating in each part of them, so as to secure concert in the muscular actions.—

4. A very important part in that respect is performed by the medulla oblongata: When it is destroyed, the co-operation of the muscles in a sustained effort is lost, even a slighter injury has the same effect, and pressure on it causes stupor.—5. The transmission of sensible impressions depends on the posterior portions of the spinal chord, the operations of volition on the anterior portions: The anterior half of the chord may be divided without affecting sensation; and the posterior half without affecting the muscular contractions.—6. In like manner the nervous chords which issue from the spine to form the spinal nerves have a separate function depending on the region of the spine, with which they are immediately connected; and of the nerves given off within the head some possess but one of the two great properties possessed by the spinal nerves, namely, either that of transmitting sensible impressions, or that of being the channel through which the muscles are excited to action, but not both properties together.—7. It is very possible that the nerves merely convey impressions, and cannot originate the influences which they convey.

As to the influence of the several masses, of which the brain consists, upon the operations of the mind and of vitality, Mr Mayo, while he admits the curiosity of the facts witnessed by Legallois, Philip, Home, Brodie, Magendie, Bell, Flourens, Rolando, Gall, and others, allows with great propriety that these elaborate researches have left us still very far from a precise knowledge of the functions of the several parts of the encephalon. The amount of the principal results appears to be,—that a wound of one side of the cerebellum causes weakness or palsy of the same side,—the removal of its upper and middle portions causes an animal to move constantly backwards,—the removal of three-fourths vertically of one side of the cerebellum, or a section of one of the crura cerebelli causes the animal to roll over and over incessantly, and towards the side which is injured, and a similar removal or section of the opposite side puts a stop to the movement,—the removal of the whole brain-proper in frogs and fishes does not impede their motion,—a section of the corpus striatum at its striated portion causes an animal to advance incessantly in a straight line,—a deep injury of one thalamus opticus causes it to turn incessantly towards the injured side,—lateral pressure of the brain or cerebellum does not cause any apparent effect, and the stupor produced apparently by vertical pressure is really owing to pressure on the medulla oblongata.

These examples will serve sufficiently, we apprehend, to uphold the opinion we have ventured to express of Mr Mayo's treatise. In conclusion, we shall endeavour to point out a few particulars which it appears to us he will have to reconsider. In the first place, he may reconsider the statement (p. 102) regarding the duration of asphyxia, and the interval after which resuscitation becomes impracticable. The experiments of Roesler noticed in this Journal (Vol. xxiii. 207,) should, we think, convince every one that the limit assigned by Mr Brodie, which is also that adopted by Mr Mayo, is incorrect. In the next place, his account of the doctrines on the decarbonization of the blood in the lungs would also need revisal: Surely the theory which supposes the carbon to leave the blood in the form of carbonic acid gas, besides being the more philosophical, is supported by very powerful and satisfactory evidence. The statement of Mr Brande, that carbonic acid is disengaged by the coagulation of blood, seems too hastily adopted by our author: Blood certainly does coagulate without giving out carbonic acid. In all likelihood Mr Brande, as well as Dr Scudamore, allowed himself to be misled by the carbonic acid given out by the decarbonization of the venous blood which they employed for their experiments.—Why should Mr Mayo prefer following Mr Brande's opinion, that the colouring particles of the blood do not contain any more iron than its other constituent principles, in opposition to the late experiments of Engelhart and Rose, which have so completely established the contrary?—(See this Journal, Vol. xxviii. 95.) We have repeated Engelhart's process, and have found his statements quite correct; serum, provided it was not reddened by colouring particles, did not contain an atom of iron. According to Mr Mayo recent blood constantly stirred does not coagulate: This is incorrect; at least the fibrine is certainly always separated in a clot. Under the head of digestion he observes, that the pancreatic fluid accurately resembles saliva, and that the gastric juice is a mixture of saliva and gastric mucus, "upon the agency of which chemical analysis throws no light." These remarks show that he had not made himself acquainted with the recent chemical researches of Leuret and Lassaigne, and of Tiedemann and Gmelin on digestion,—both of which, however, were published in 1825. This is farther shown by his passing over so cursorily the function of the cœcum, and by his confining to the investigations of Prout his remarks on the changes which the food undergoes successively in its progress through the alimentary canal. On perusing the works in question, he will probably agree with us in thinking, that his whole chapter on digestion will require remodelling. Among other particulars, he will find his own experiments respecting the influence of the bile on chylification contradicted by both the German and the French experimentalists. —In his enumeration of the proximate animal principles which are digestible he has unaccountably omitted casein.—In describing the sources of animal heat, he very correctly adopts the view that the chemical phenomena of respiration will not account for all the heat generated. The experiments, however, on which he rests his opinion, those of Mr Brodie, are far from conclusive; and he has omitted those of Despretz, which are not only quite conclusive, but likewise point out the proportion accounted for by the respiration in different tribes of animals, and are, moreover, well worthy of notice as the most accurate and best-devised experiments hitherto performed on this interesting subject. We may remark in general terms that the chemical department of physiology is the one in which our author seems to feel himself least at home.

There are two important omissions which it would perhaps be well for him to supply. The first is the mechanism of the various muscular motions, on which he is too brief, considering its close relation with surgery, and the little attention usually paid to it by students. The other is the question regarding lymphatic absorption lately revived by the alleged discoveries in Italy regarding the anatomy of the lymphatic system,—a question on which we should have been well pleased to have had the detailed opinion of so accomplished an anatomist as Mr Mayo. Lastly, we would recommend to him, as a valuable addition to every elementary work, a much more copious and precise list of references.

Mr Mayo we hope will not misapprehend the object of these strictures. We have no hesitation in saying that his book ought to be in the hands of every student.

ART. III.—1. *Exposé des Divers Procédés, &c.—Account of the different methods employed to the present time to cure Stone without having recourse to the operation of Lithotomy.* By J. LEROY (D'ETIOLLE,) M. D. Paris, 1825.

2. *Remarques sur l'Uretre, &c.—Remarks on the Urethra, Male and Female.* By M. AMUSSAT. *Archives Generales,* Tome iv. January 1824.

3. *De la Lithotritie, &c.—On Lithotrity, or Destruction of Stone within the Urinary Bladder.* By Dr CIVIALE. With five plates. Paris, 1827, 8vo. pp. 254.

4. *Rapport, &c.—Report presented to the Royal Academy of Sciences, by* MM. CHAUSSIER and PERCY, *on the method proposed by Dr Civiale for destroying Stone, without having recourse to Lithotomy.* Paris, 1824.

5. *Memoire sur la Lithotritie, &c.—Memoir on Lithotrity.* By Alphonso Tavernier, D. M. P. *(Journal des Progres des Sciences et Institutions Medicales en Europe, en Amerique,* &c. Vol. II. Paris, 1827.*)*

It is by no means wonderful that at all periods of the history of the healing art it has been a favourite problem to discover some means of removing urinary concretions less formidable than that of lithotomy. The pain, the danger, and the uncertain result of all incisions made into the urinary bladder are sufficient to invest every form of the operation with more or less terror, and make it be viewed at all times by the patient, if not by the operator, with that feeling of uncertainty from which none but a very ignorant or a very indifferent person is wholly exempt. Though the surgeon may look to the operation of lithotomy with that alacrity and pride which spring from a conscious sense of the triumph of his art, to the patient it appears as the only alternative of a life of pain and misery, and the only probable mode of removing a disease which in some instances is worse than many deaths.

The first proposal by which the evils, real or supposed, of the surgical operation were attempted to be avoided was that of medicines, which were confidently called lithontriptic. This idea, however, which originated in ignorance, not only of chemical, but of physiological laws, was, after many futile and unavailing trials, abandoned as erroneous. It is needless now to revive the history of the argument, the ridicule, the invective, and the irony, with which the reputation and the memory of the unhappy Mrs Stephens were alternately assailed. These were the days neither of Black nor Lavoisier; and if the calcined egg-shells and Alicant soap did no other good, they led Whytt and others to the notion of lime-water, and the whole train of alkaline remedies. The system has been simplified, and the theory has been rendered intelligible; but at the present day the efficacy of the alkaline remedies is maintained by Sir Gilbert Blane, Mr Brande, the late Dr Marcet, and Dr Prout.

As the great objection to this hypothesis consisted in the fact, that no lithontriptic of sufficient strength to operate on a stone in the bladder could be conveyed into the stomach without more or less injury to that organ, and that in such a form as not to affect the stomach, it must be inadequate to operate on the urinary concretion, and was liable to decomposition. To obviate this objection it was proposed to convey the lithontriptic medicines directly into the bladder. The practicability of this method, which was understood to have been originally suggested by Hales, was maintained in 1742 by Makousky of

Konigsberg, and demonstrated in 1746 by Browne Langrish on brute animals,* and about ten years after by William Butter on the human subject.† The latter gentleman, then a clerk in the Royal Infirmary of this city, injected lithontriptic solutions into the bladders of two patients labouring under symptoms of stone in that establishment in the presence of Dr Whytt, and published a description of the apparatus which he conceived most suitable for the purpose. No further account, however, was given of the results of these injections; and the practice appears to have been left to languish in neglect, till revived under happier auspices by Fourcroy and Vauquelin in 1791. These distinguished chemists detailed in the second volume of the Memoirs of the Medical Society of Emulation the particulars of several cases in which they tried various injections. From these trials, and from the recent testimony of Marcet, who had also put it in practice, it must be admitted that the method by injection possesses the merit of being more consistent with the laws of chemical action than the former. It has not, however, been free from considerable difficulties, which have tended to keep it rather in the place of a scientific curiosity than in that of a practical and useful measure susceptible of general application. Even the continued injections by the means of the double tube imagined by Hales,‡ and afterwards realized by Gruithuisen,§ though recently applied by the ingenuity of Magendie, have not afforded results perfectly successful.||

In short, the method is beset with difficulties so considerable as to render its general adoption as a surgical remedy exceedingly improbable. The necessity of obtaining accurate information on the nature of the concretion, and of accommodating the solvent to the substance to be dissolved, is indispensable to its success, and even to its innocence; and this is by no means easy even to chemists, and very difficult to ordinary practitioners. It is further to be remarked that no fact is better ascertained than that the saline, or earthy matter deposited from the urine, varies at different periods in the same individual; and that in the same concretion three or four different kinds of substance may be found. Lastly, even supposing the case to be simplified by the removal of these difficulties, the operation of

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* Physical Experiments upon Brutes in order to discover a safe and easy method of dissolving the stone in the bladder by injections, &c. Read before the Royal Society. By Browne Langrish, of the College of Physicians, and F.R.S. London, 1746.

† A method of cure for the stone chiefly by injections. With descriptions and delineations of instruments contrived for these purposes. By William Butter, M.C. Edinburgh, 1754.

‡ Hemastatics, p. 175.

§ Salzburg Zeitung, 1813.

|| Leroy, Exposé, &c. p. 96.

injection requires to be so frequently repeated, and so long continued, before any material impression is made on the concretion, that few patients can be found who have sufficient firmness and patience to persevere with a process which, conducted with the utmost caution, is attended with more or less irritation of the urinary passages.

An application of chemical agency, still more elegant and ingenious, originated with M. Bouvier Desmortiers, who first conceived the possibility of dissolving urinary concretions by the decomposing influence of galvanism. This happy idea was adopted by Gruithuisen, who in 1813 undertook to prove its practicability in regard to concretions out of the bladder. The experiments which he made led him to infer that almost no concretion resists the action of a pile of 300 plates, which have the effect of forming deep holes in its substance; and he advises in the case of a hard calculus to employ 600 plates, when it would be dissolved with remarkable rapidity.

This method of destruction was in 1823 more completely investigated by MM. Prevost and Dumas. They first subjected a fusible calculus out of the body to the operation of a pile of 120 plates, which was charged afresh every hour; and they found that a concretion, which at the commencement of the experiment weighed ninety-two grains, was reduced in twelve hours to eighty grains. In sixteen hours more it was so friable that on the slightest pressure it fell into crystalline grains, the largest of which did not equal the size of a lentile. In a second experiment, performed on the fragment of a concretion introduced into the bladder of a living dog, in the course of six hours they rendered it very friable, and deprived it of much of its weight, without apparently causing the animal much pain.

By M. Leroy the apparatus employed by MM. Prevost and Dumas is believed to labour under some disadvantages, especially in the conducting wires being retained irregularly in contact with the concretion. This inconvenience he proposes to obviate by first grasping the stone by an instrument analogous to what he terms the *lithoprion*, and having entirely or partially perforated it thus secured by connecting one wire with its surface, and the other with the hole in the centre, the stone, assailed in this manner at two points, must speedily give way; while the bladder cannot be injured by the galvanic current, which is totally expended on the concretion. The suggestion is certainly highly ingenious; and as by its scientific precision it obviates the principal objection to which the galvanic method of decomposition is liable, it has a just title to more extensive trial than it has yet undergone.

A third mode of obviating the alleged evils of cutting into the bladder to extract a stone is that of mechanical destruction or division into fragments so minute that they may be expelled through the urethra. At first sight this proposal seems the most impracticable of any, and calculated to be little more than the pastime of ingenious but chimerical speculatists. The narrowness and unyielding nature of the male urethra, its length, its curvature, and the delicacy of the textures of which it consists, all present obstacles at first sight utterly insuperable,—*first*, to the division of such a substance within the bladder as a urinary concretion ; and, *secondly*, to the extraction of the fragments by the urethra without serious, if not irreparable injury to that canal.

Such are the thoughts of preconception unregulated by the knowledge of actual facts, and the results of accurate experiments. Difficult and chimerical as the proposal may appear, it has actually been realized ; and if there be truth in human testimony, it cannot be denied that stones have been so much divided and broken by mechanical contrivance within the bladder, as to be extracted through the urethra not only without injury, but with comparative ease.

The merits of originality in contriving this method of removing urinary concretions from the human bladder is claimed by different individuals. It would be foreign to our purpose to enter into the question of the rights of either of the claimants ; nor do we propose to espouse the cause of any one of them. The general merits of the contrivance, admitting its utility and practicability to be equally great as they represent it, are very much divided. It is enough to say, that all seem to be agreed that to M. Amussat belongs the chief merit of urging and demonstrating, at least at present, from accurate knowledge of the structure and configuration of the urethra, the practicability of using the necessary instruments. M. Leroy again appears to have been best acquainted with the history of the operation, to have known most accurately what had been imagined and executed in all that relates to the extraction of vesical calculi, without recurring to cystotomy, and to have proceeded on the most rational plan for rendering the method one of experience and practicability. Lastly, the name of M. Civiale is still most closely connected with the actual performance of the operation, several, we believe now many, examples of which he has successfully accomplished on the living subject.

After these preliminary remarks, we proceed to take a short view of the steps by which this operation of destroying calculi within the bladder has been gradually brought to its present state ; and to consider briefly the different methods by which the object is believed to be most readily and expeditiously attained.

The removal of concretions from the male bladder without recurring to incisions implies three conditions. The first is, that the urethra be susceptible of a certain degree of dilatation. The second, that the stone, or fragments of stone, be very small; in other words, that it be reduced to minute portions. And, with a view to the accomplishment of this effect, the third condition is, that the direction of the urethra admit of the introduction of instruments adequate to divide the concretion if necessary, and to facilitate the expulsion of its fragments through the canal.

It is unnecessary to advert to the spontaneous escape of concretions from the bladder through the female urethra. That fact is so well established by cases recorded by various authors, more especially by Bartholin, Borelli, Kerkringius, Grunewald, Morand, and those given by Drs Wallis, Gardner, Beard, Molyneux, and Leprotti, in the Philosophical Transactions, that it is equally impossible to doubt it, and superfluous to undertake its demonstration. It is well known that upon the knowledge of this fact depends the method originally practised by Stromani and Franco, * revived by Tolet, † and Bromfield, ‡ and repeatedly performed of late years by Sir Astley Cooper § and others, ‖ of extracting calculi from the female bladder by dilating the short and distensible urethra by the introduction of sponge-tents and similar mechanical contrivances.

Very different is the case with regard to the urethra of the male; and the spontaneous escape of concretions by this canal is of infinitely less frequent occurrence. Instances, however, even of this event have occasionally happened; and all who are acquainted with the history of vesical surgery, or are conversant with its practice, are well aware of cases of this description. Almost every collection, public and private, contains many examples of minute calculi, more or less numerous, which have been discharged under favourable circumstances by the urethra; and most practical surgeons have met with cases in which patients continued during many years to expel calculi through that canal from the bladder.

These facts, together with the operation of the steel sound, are sufficient to show that the male urethra is dilatable to a certain degree. M. Leroy states five lines to be a diameter attained with difficulty, and thinks that he is justified in inferring that many men can with difficulty support sounds of this

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* Traité tres ample des Hermes, p. 143. † Traité de la Lithotomie.

‡ Surgical Observations and Cases.

§ Medico-Chirurgical Transactions, London, Vol. viii. p. 433.

‖ Drs Hamilton and Ramsay in Medico-Chirurgical Transactions, Edinburgh, Vol. ii. and Dr Mackintosh in Med. and Surgical Journal, Vol. xxiv.

size. This statement may be correct in reference to the vesical extremity and the prostatic portion of the canal; but in other parts it is certainly more dilatable, at least in the average of well-sized healthy adults. The steel sound marked No. 14, we find to be exactly five lines in diameter, and not only this but No. 15 we have repeatedly introduced in the adult male, and others have done the same. Upon this point we expected to find some exact information in the essay of M. Amussat; but that gentleman has been more solicitous to establish the flexible nature of the male urethra than to ascertain to what extent it was dilatable.

But previous even to the lights afforded by correct anatomical science, the dilatability of the male urethra appears to have been believed, if not demonstrated. We have the evidence of Prosper Alpinus to show, that among the Egyptians in the sixteenth century,* it was an ordinary practice to distend the urethra with air, in order to extract urinary concretions. The same writer states that he witnessed an Arabian physician extract in this manner calculi as large as olives, or even small nuts.† At a much later period in Europe, it appears from the writings of Meckren and Helwig, that the method of mechanical dilatation was successfully practised for the same purpose.

It is nevertheless well established, that beyond a certain point the male urethra cannot be dilated; and hence, in several of the instances in which calculi are recorded to have been detruded from the bladder into the urethra, they have stuck fast in the canal until removed by art. This accident appears to have been noticed at an early period; for we find in the writings of Albucasis, Hildanus, Ambrose Paré, and others of the older surgeons, frequent mention of cases in which it was requisite to extract the stone from the urethra, and minute descriptions of instruments and mechanical contrivances for effecting this object successfully.

" But if a stone stop," says Paré, " in the end of the *glans*, it must be plucked out with some crooked instrument; to which, if it will not yield, a gimblet, with a pipe or case thereunto, shall be put into the passage of the yard, and so it shall be gotten out, or else broken to pieces by the turning or twining about of the gimblet, which I remember divers times to have attempted and done; for such gimblets are made with sharp screws, like ordinary gimblets."‡

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* Alpinus tells Guilandinus that he sailed for Cairo in 1580, and left Egypt in 1584. De Medecina Ægytiorum, libri iv. lib i. x.

† Ibid. lib. iii. chap. 14.

‡ The works of that famous chirurgion Ambrose Parey, London, 1634, Lib, 17, chap. xxxix.

That the operation of Paré was afterwards practised by Daniel Fischer of Kesmark we learn from his son Charles Daniel, who has left an account of the method in which his father operated. This surgeon, whose aid was requested to relieve a Hungarian counsellor of noble rank from the sufferings occasioned by a concretion impacted in the urethra, having caused a steel canula containing a piercer to be constructed, not to break but to perforate the calculus, introduced this instrument, moistened with almond oil, to the concretion, which he secured by holding the urethra with his left hand. When he satisfied himself that the canula was applied to the surface of the stone, he introduced his piercer and proceeded to work it until the concretion was thoroughly perforated. This being accomplished, the piercer was withdrawn, and a pair of long-beaked small forceps was introduced so accurately as to enter the hole made in the calculus. The handles of this instrument being then separated, the stone was accurately broken in four pieces, which were voided when the patient made water with little pain; *exiguo cum dolore;* and the counsellor was henceforward completely delivered from this evil without any injury of the urethra, *absque omni læsione urethræ.*[*]

We give this case as a good example of ingenuity in surgical operation, and to show that the idea of lithotrity, at least in the urethra, is by no means new; and we believe that few will dispute that the operation of Fischer is a valuable improvement of the method of Ambrose Paré.

This operation, nevertheless, does not appear to have been since repeated; and surgeons have been contented to employ forceps, pliers, and hooks, for the purpose of extracting stones from the urethra, or to facilitate the operation of these means by the use of bougies and mechanical dilatation. The former was the method conceived by Hales, and practised by Ranby, Lamotte, Hunter, Desault, and other surgeons. By means of the latter, Boyer succeeded in extracting four calculi of good size from the urethra of a man of sixty years.[†]

Of the same practice a variety still more advanced is that of extracting small calculi from the bladder through the entire length of the urethra. This method, the merit of which has been generally given to Sanctorius, has not, however, been much followed. Sir Astley Cooper alone several years ago removed in this manner from the bladder of one patient not fewer than eighty-four concretions of the size of garden peas.[‡] The instrument used by this dexterous surgeon was a pair of two bladed bullet-forceps, made to open when in the bladder, by means of a stilette, so as to grasp and confine the stone during extraction.

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[*] Dissertatio Medico-Chirurg. de Calculo Vesicæ Urinariæ in Urethram impulso et singulari Encheiresi, absque sectione exempto. Quam palam tuebatur Carolus Daniel Fischer, Kesmarkkia-Hungarus, 1744. Haller, Disp. Chir. Tom. iv.

[†] Traité des Maladies Chirurgicales, &c. Tome ix. p. 318.

[‡] Medico-Chirurgical Transactions, Vol. xi. p. 349, 357.

When the disease consists of numerous small stones, it is obvious that the chance of their expulsion from time to time, or even their extraction, is much increased. The expulsion of one stone exceeding a certain size is an anatomical impossibility; and the first difficulty to be surmounted in the removal of such a concretion is its size. To divide it therefore, or break it down by mechanical means, is the most obvious method of attaining the object; but how to affect this through such a narrow canal as the urethra, was the point which appears to have perplexed all those who investigated this subject, and most powerfully called for the exertion of their ingenuity.

In meditating on these difficulties, we have a good example of what has occurred more than once in the history of the healing art. We allude to the circumstance, that a measure, from the adoption of which a professional person regularly educated, impressed, as he imagines, with the principles and reasons of all his practical proceedings, and understanding, as he conceives, their dangers, difficulties, and practicabilities, would shrink in despair, recoil with indignation, or turn with absolute contempt, is not unfrequently confidently undertaken, and successfully accomplished by one utterly ignorant of the supposed principles, utterly unaware of the imagined dangers, and in all probability completely incapable of comprehending the alleged practicability. The operation of breaking down a stone within the bladder was first performed, so far as testimony goes, by an ecclesiastic of Citeaux, and a military officer of the East India Company's service.

The latter case, that of Colonel Martin, Dr Monro, the present Professor of Anatomy in the University of Edinburgh, used to relate to his pupils as an instance of a method of curing stone without cutting into the bladder,—singular in its nature, and therefore worthy to be known, but though practicable, neither very expedient nor likely to be repeated. In alluding to this fact, the reporter speaks as if its statement required to be clearer, and its date more distinctly fixed, in order to establish its title to priority. We pledge our personal testimony for it so long ago as 1813; and the case had then been related much oftener than the first time, so that the professor is as much entitled to the merit of the method of lithotrity as any of the present competitors. The case has since been published in the Journal of the Royal Bombay Institution, and again in the 2d edition of the work of Dr Marcet, in either of which works an account may be read. The method used by Colonel Martin was by introducing through the urethra a steel sound, the extremity of which was sharpened like a file. For an authentic account of this we refer to another department of this number.

To this case the authors of the report on the work of M. Civiale have added one which appears to have occurred at a still earlier period. A monk of Citeaux in Normandy is said to have cured himself of stone by introducing into his bladder a flexible catheter, through which he caused a long straight piece of steel, terminating like a chisel, to move. When the end of this was brought against the stone, he struck smart blows on the other end with a hammer, and thus succeeded in detaching minute portions of the calculus, which, when voided, were preserved for the inspection of the curious. The authority on which this narrative rests, is not mentioned; and we must avow our ignorance of its original source.

The practicability of the method of lithotrity was thus demonstrated empirically long before its merits were examined, or its principles were investigated by surgeons.

An important element in any scheme of this description is to ascertain the practicability of breaking down a concretion within the bladder by instruments introduced through the urethra. Though not absolutely impossible, it may be attended with such difficulties and inconveniences as to render it inexpedient; and this itself may constitute a serious objection to the lithotritic method. It is manifest, that, in the case of hard concretions, such, for instance, as consist chiefly of lithic acid, of phosphate of lime, or of oxalate of lime, the difficulty of dividing them mechanically is much augmented. In the softer species again, especially the fusible calculus, which is friable, and the ammoniaco-magnesian phosphate, which is always less compact than the three first mentioned, division by instruments must be comparatively easy, and of short duration.

It is well ascertained that some urinary concretions have fallen down spontaneously into fragments within the bladder; and various examples of soft or friable calculi are recorded or alluded to in the annals of vesical surgery. Barbette stated long ago that he had more than once met with calculi so soft, that he was persuaded he could have broken them even with the sound in the neck of the bladder.* Heister, in the Philosophical Transactions, (No. 417, p. 13,) describes a case in which 100 pieces of broken calculi were at different times voided through the urethra. And Dethardring† has recorded an instance of fragments of a calculus being voided for many weeks. Lastly, it is well known to those conversant in surgical operations, that in several instances in which lithotomy has been performed, the concretion has been so friable as to break down between the blades of the forceps, and increase greatly the labour of the operator in extracting them.

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* "Se tam mollem sæpius invenisse vesicæ calculum ut etiam vel solo cathetere in collo vesicæ conterere potuerit."—Chirurgia, p. i. c. 76.

† Dissertatio Medico-Chirurgica de Calculo Vesicæ Friabili. Apud Haller, Disput. Chirurg. Tom. iv.

While these facts show the difficulty of the method of lithotrity in some instances, they demonstrate its practicability in others. If it can be ascertained that the stone consists either entirely or chiefly of the fusible matter, (triple phosphate and phosphate of lime,) or of the triple phosphate, or even of the cystic oxide, it may be very easily broken down into small portions.

To Gruithuisen, the Bavarian physician, above-mentioned, is ascribed the merit of conceiving this mode of operating, and of contriving an instrumental apparatus for effecting it. This physician commenced his proposals by demonstrating not only the practicability but the facility of introducing straight sounds through the urethra into the bladder. In two individuals he introduced, without difficulty, glass cylinders rounded at the ends, and from three to four lines in diameter. He afterwards, to obviate the objections of the incredulous, performed the same operation on a man of thirty, in the presence of Counsellor Mussinau, Professor Kaurk, and other competent judges.*

The point of straight catheterism being established, Gruithuisen proceeded to describe his lithotritic apparatus, which consists of a silver canula enclosing a double wire loop for clasping and holding the stone, and a sharp piercer or stone-drill, which, being made to revolve on its axis by means of a bow, is made to perforate the part of the stone to which it is applied. In some instances he proposed to vary this instrument by substituting the crown of a small sized trepan.

The method now mentioned is with some justice censured by M. Leroy, who regards it as too dangerous a mode of dividing or comminuting the stone. It is certainly a mode both rude and hazardous, in so far as a wire noose, not very accurately fitted, is much too insecure a means of retaining a concretion upon which a perforating instrument is operating within the bladder. He acknowledges, however, that to Gruithuisen is due the merit of opening the path to this new mode of operating.

It is a remarkable proof of the ignorance in which at certain times one part of the world remains of the proceedings of the others, that this proposal of Gruithuisen seems to have been completely unknown in France, and if known in this country, no proof of such knowledge ever was given. This at all events may be inferred from the fact, that the next proposal of this description is that of Mr Elderton, originally published in this Journal, Vol. xv. This contrivance, for the account of which we refer to its proper place, is admitted by M. Leroy to show that its author perceived the practicability of lithotrity, but is still objectionable from the insecure mode in which the concretion is proposed to be held.

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* Salzburg Zeitung, 1813.

Previous even to this period, according to the statement of the report, that is, in July 1818, M. Civiale had applied to the minister of the interior for pecuniary advances to defray the expence of constructing instruments, which he conceived would be adequate to destroy urinary concretions within the bladder without recurring to cystotomy. This application was some days after referred to the Faculty of Medicine, with a memoir explaining the instrumental apparatus, which, even at this stage of the business, he had denominated *lithontriptor*. On the 14th of the same month the society appointed to M. Civiale the same commissioners who were latterly given him. But at this period no report was made, and the matter remained stationary.

The following year, however, 1819, the lithontriptic apparatus was constructed by a mechanician of Paris, with the modifications and amendments which it possessed at the date of the report. The invention of the instrument, therefore, described and employed by M. Civiale, is assigned to that gentleman exclusively by Baron Percy and M. Chaussier, the authors of the report. Admitting the ingenuity and originality of the method of the Bavarian physician, they consider it, nevertheless, vague and incoherent; and they assign to M. Civiale the first place for the happy manner in which he has established, explained, and put in practice a proposal little more than sketched in a foreign journal.

The first step which appears to have occurred to M. Civiale at this period was the possibility of introducing a straight sound or catheter into the bladder. Baron Percy remarks, that De La Sône, in studying the anatomical peculiarities of the urethra, had recognized that this tortuous but extensible canal might be made to follow several directions, and become even rectilineal under the use of a sound of this shape. The experiments of Gruithuisen, and the instrument of Elderton already mentioned, were equally unknown. Meanwhile, M. Amussat, who openly claims priority on this point, had, from his intimate knowledge of the disposition, direction, and capabilities of the male urethra, been in the habit of employing straight catheters previous to 1818. We have already, on a former occasion, mentioned this surgeon and the peculiarity of his method; and it is superfluous at present to do more than merely refer to the article in which his name is introduced.* Though we are aware that some experiments on this point had been performed in London by Mr Stanley, teacher of anatomy, yet we cannot speak positively as to the exact date, whether it be previous or subsequent to that at which M. Amussat had established the fact. It cannot be denied, on the whole, that to the latter belongs the merit of clearly demonstrating the fact, and acting on its knowledge in his operations on the bladder and urethra. He has in short rediscovered the method practised by Gruithuisen.

In returning to M. Civiale, we now find him interfering somewhat with the claims of M. Leroy. It appears to be quite undetermined whether the former gentleman had also established, by his own researches, the possibility of introducing a straight catheter, or had derived this from M. Amussat. According to the statement of M. Leroy it appears, that while M. Amussat was led by his employment of the straight sound to contrive a stone-breaking instrument, M. Leroy, pursuing this discovery of M. A., had also effected a similar invention. M. Leroy, however, it must not be forgotten, connects the announcement of M. Amussat of the practicability of using the straight catheter with the month of April 1822, a date nearly four years posterior to the time when this anatomist is said to have first established the fact, and three years posterior to the time when M. Civiale is said to have constructed his lithotritic instruments.

These circumstances render it difficult to decide the question of priority as to the invention of the *brise-pierre*; and perhaps, though important to the individuals concerned, it is of no great moment to the surgical world at large. The fact is certain, that both M. Leroy and M. Civiale have contrived apparatus adequate to break down urinary concretions within the bladder; and this is probably all that it is important for the world at large to know. For a fuller view of this debated point we refer the reader to the elaborate paper of M. Tavernier. We come now to consider the comparative merits of the methods in use, and to examine how far they are calculated to attain the end in view.

The principal instrument devised by M. Leroy is what he terms the *lithoprion* or stone-saw, (λιθος *lapis* and ϖριων serra;) of which he has two forms,—the simple lithoprion, and the lithoprion pincers. The essential part of the former is a steel rod, to one end of which are attached two small saws, mutually diverging by means of a spring, and the toothed edges of which look in opposite directions. This instrument is to be introduced through a silver canula, which is enclosed within another canula, in the interval between which are contained four pieces of watch-spring, so arranged that when introduced into the bladder they start out and embrace the concretion, which is thus maintained immoveable. When the stone is thus secured, a long drill is introduced through the inner canula, and by working it upon the stone, a hole is drilled as nearly as may be through the centre. This being completed the drill is withdrawn, and the lithoprion is introduced in its place, and, being inserted into the perforation made by the drill, is wrought in the interior of the stone, until the latter is either entirely divided or easily gives way.

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* Medical and Surgical Journal, Vol. xxiv. p. 172.

With this apparatus M. Leroy made many trials on the dead subject, and two of these publicly. Each time the stone was seized and perforated without causing any injury to the bladder; and the experiment was carried no further, since the stone, which consisted of oxalate of lime, was large and very hard, would have taken too much time to be entirely broken, and because in the dead body the absence of sensibility must have rendered the operation the same from the beginning to the end.

The resemblance between the lithoprion of M. Leroy and the instrument of Gruithuisen in general principle is obvious. It must be admitted, nevertheless, that the steel-spring-case affords a much more secure method of clasping the stone and preventing injury from the drill than the wire loop of the latter author.

Some physicians thought they could trace an analogy between the lithoprion and the *vesical* of Franco, or the ball-screw of Alphonso Ferri. Though M. Leroy contends that there is a great difference between it and these instruments, he has not disdained to imitate the principle of the ball-screw in constructing what he denominates the lithoprion pincers or forceps. He also admits the resemblance of this instrument to the three-fanged pincers of Sanctorius. It consists of a steel canula terminating in three tempered fangs, which are made to separate naturally. By sliding this into a silver canula these fangs are made to approach mutually, in order to be introduced through the urethra into the bladder, when, by moving the steel canula according to the indications of a scale attached, they may be separated to any degree till they grasp the stone. Their concave surface is armed with filing or sawing teeth; and by withdrawing the steel canula a little they are brought to clasp the surface of the concretion. A hole being drilled in the centre of the stone as before, a bifid drill is introduced and carried through the perforation; and by giving this a rotatory motion, either the stone is carried round, while the surface is rasped by the lithoprion forceps, or if these grasp it too firmly to admit of this motion, the central hole is enlarged, until the stone is reduced to a thin shell, which at length breaks into fragments.

Such is the apparatus of M. Leroy, and its mode of operation. The details regarding the mode of procedure, and the rules to be observed, are also given; but these the imagination of the reader will easily supply.

Of the apparatus *peculiar* to M. Civiale it is not so easy to obtain a distinct account. At first his object was to dilate the urethra, to introduce into the bladder a straight instrument, in order to perforate the calculus, and to enclose it in a sac containing a solvent liquor to effect the solution, as well as the mechanical division of the stone. With this view he constructed an instrument furnished with a pouch or sac, and forceps with four arms, carrying in its interior a sort of drill, which was made to revolve by the fingers. This second instrument, termed indifferently *lithontriptor*, *lithoprione*, or *saxifrage*, was constructed of two hollow metallic cylinders, one of which was inserted within the other, and the end of which was furnished with four or more diverging branches kept apart by their own elasticity. Their inner surface, nearly flat, was smooth in the two posterior thirds, but armed with teeth in the rest. The opposite end of this cylinder was marked with lines to indicate the degree of divergence of the arms when the instrument is in the bladder. It was fixed to the outer tube by means of a screw attached to the latter. Through the inner cylinder was introduced a steel stilette, the anterior extremity of which is pyramidal, with acute angles or flat, but armed with teeth like those of a saw; and by giving this a rotatory motion the concretion was to be perforated or ultimately broken.

The first part of this apparatus, the pouch-instrument, is clumsy and inconvenient, if not impracticable in application. The second part, the lithontriptic instrument, is better, but is nevertheless so imperfect, that we believe it has since been relinquished by its author; or at least it has undergone changes so great that it can no longer be viewed as the same instrument.

Thus to the blades of his lithontriptor, which were formerly flat, he has given a degree of curvature, so as to form a concave receptacle for the concretion. He has armed them all over with teeth, so as to operate on the stone at all the points of contact. And lastly, he has given the drilling stilette a more energetic and rapid rotatory motion, by subjecting it to the action of a bow or a hand-drill. In short, the instrument now used by M. Civiale is as near as may be the latter instrument or the lithoprion forceps of M. Leroy.

These several forms of lithontriptic apparatus have been more or less modified and improved, according to the views of individual operators. Of these the most distinguished are M. Heurteloup and M. Merieu, who have effected, in the construction of the instruments, some rather beneficial changes, by which the operation is certainly both simplified, rendered less dangerous, and made considerably easier. Into the details, however, of these changes we have not leisure to enter.

The operation of lithotrity itself has been divided by those who practise it into three stages. The first is that in which the straight instrument is introduced and the calculus is seized and secured. The second is that employed in perforating, rasping, and breaking it down. To the third they have allotted the business of extracting the fragments from the bladder.

In the different modes of proceeding by Leroy, Amussat, Civiale, Heurteloup, and Merieu, the principle of securing the stone is much the same. That of M. Heurteloup, as described by Tavernier, appears to be the most efficient. Like the others, it consists of a four-bladed forceps, placed in the interval between two silver tubes, one of which is inserted within the other. That of M. Merieu is distinguished first in the two tubes being made of steel, in the inner one being incomplete its whole length, and split into five portions, which form so many blades of forceps, while the deficiency is supplied by two other plates, which are made to form the sixth blade; and secondly, in his lithontriptic instrument or stone-file being made to act at once in the centre and on the circumference of the concretion.

The destruction of the stone is effected in all the methods, first, by drilling a hole in the centre, as nearly as it is possible to conjecture, of the concretion; secondly, by enlarging this in various modes.

To effect this M. Leroy introduces the bifid rasp closed into the perforation, after which, by withdrawing the canula, the arms are allowed to diverge. The canula is then secured by means of the screw, and the instrument is subjected to the rotatory motion by the hand-drill. When the bifid rasp has thus enlarged the opening as much as the divergence of its limbs admits, a simple rasp, supported by an elastic stalk, is introduced, and moved in the same manner; and successively other rasps may be used till the operator is satisfied that the stone is reduced almost to a shell.

In the method of M. Merieu, when the concretion is seized, the stone-file *le lithorineur* (λιθος *lapis* ϱινη *lima*,) an instrument consisting of a central piercer and two files placed at the same angle to it, is made to revolve by means of the hand-drill, and while a hole is drilled in the centre, a spherical segment all round is removed from the surface of the stone.

In the method of M. Heurteloup, when the concretion is perforated, an instrument termed a *scooper*, armed with a serrated plate, is introduced into the perforation; and by the rotatory motion communicated to it, while the obliquity of its direction is by particular mechanism constantly increased, as the destruction of the stone proceeds, it acts from the centre to the circumference until the stone is reduced to a mere shell, and is broken with facility.

In the method of M. Amussat, after the perforation is completed by the first drill, its place is supplied by another, which is bifid, and the two arms of which, armed with cutting edges, are separable to any given extent by a simple mechanism. The handle being applied, the rotatory motion is continued until the operator feels that it no longer grates the hard matter of the stone, when the arms of the bifid drill are again to be separated. The rotation is resumed until so much of the stone has been grated that the sound again becomes faint or ceases, when the arms of the drill are once more to be separated, and the same course continued. When the stone thus grated from the centre to the circumference has become very thin, if it does not give way at once by the repetition of the drilling, it is to be seized with the stone-breaker, (*brise-pierre*) and crushed between its toothed blades.

The mode of removing the fragments is similar in all. The mere powder is carried off by the urine, or by injections of watery demulcent fluids. Small fragments are extracted by such contrivances as the forceps of Hunter; and if fragments too large for this are still left, a second division is necessary.

In addition to the instruments of the surgeons above-mentioned, Mr Weiss of London, whom we have had occasion to mention more than once as an ingenious artist, has contrived a lithontriptic instrument; and Mr Lukens of Philadelphia has in like manner constructed for the same purpose what is there termed a *lithokonion*, a description of which is given by Dr Horner in the Philadelphia Journal, (Vol. i.)

It is rarely possible to destroy a urinary concretion by the first attack, whatever be the method employed, more especially if the stone is hard, if it exceeds an inch in diameter, and if the patient is irritable. M. Civiale does not detain his patient much beyond ten minutes. The number of times which the operation requires to be repeated varies according to the size and the consistence of the stone, the wearing which it causes to the instrument employed, and upon the state of the patient. Equally variable are the intervals between the operations. M. Civiale believes it may be repeated at a space varying from the third to the fifth day.

It would doubtless be very desirable if this repetition of the operation could be entirely superseded, and if a concretion of any size could be completely reduced to fragments at a single sitting. A hope of this description is held out by some of the apparatus employed, especially by that of M. Merieu. But whatever power and efficacy be given the instruments employed, it is manifest that nothing but actual experiment can determine the length of time during which the necessary apparatus can be retained in the urethra without injury to that canal, or the neck of the bladder itself.

The report of Baron Percy contains the details of three cases, in which M. Civiale destroyed and removed entirely urinary concretions within the human bladder, in the presence not only of the reporters, but other eminent physicians and surgeons of the French metropolis. Among others the reader recognizes the names of Larrey, Giraudy, Magendie, Sedillot, Serres, Nauche, and Souberbielle,—sufficient testimony to the authenticity of the facts. A brief outline of these cases may be interesting to the reader.

In the first case a young man of thirty-two, proved to have in his bladder a large hard stone, by sounding in the presence of the reporters, and Larrey, Giraudy, and others, was the subject of operation on the 15th January 1824. The moment the bow was put in motion all present heard a grating noise, which was at once ascribed to the hardness of the mulberry caculus, (oxalate of lime.) The operator allowed a respite thrice to the patient, who felt rather constraint than actual pain. After forty minutes the patient descended from the bed, and with a little urine voided the water injected, and discharged numerous fragments of the concretion, which was supposed to be diminished one-third. On the 23d the operation was repeated in the presence of the reporters and MM. Magendie, Serres, and Aumat; and again on the 3d of February, when after considerable quantity of powdery matter and fragments was discharged, the patient was ascertained to be free of the stone.

The second case was that of a man from Rheims, whose bladder was ascertained to contain a concretion, the nucleus of which was known to be a garden bean. From the sounding, which was performed in the presence of the reporters and Souberbielle, a well known and accomplished lithotomist, the concretion was judged to be soft and about the size of a chestnut. On the 4th of February the instruments were introduced, and the stone seized. The sound heard was dull and occasionally obscure. The bladder, however, being very irritable the operation was abridged; and the application of leeches and the injection of emollient liquors were deemed expedient. The result was the discharge of several friable portions of stone and much earthy sediment. The operation was repeated on the 7th with the effect of causing the discharge of some portions of divided concretion, and two or three little masses of viscid animal matter. At a third operation on the 10th the forceps seized something not very large and somewhat compressible, which proved to be the bean stripped of its earthy crust, and presenting a rostellum as if germinating. Some days after the husk of the bean was clasped by the forceps and extracted; and a last fragment, the presence of which was ascertained by the accurate sounding of M. Souberbielle, was easily removed by M. Civiale by means of the forceps of Hunter.

In the third case, a young Parisian was on the 2d of March ascertained in the presence of the reporters, M. Souberbielle, and other surgeons, to have a stone about the size of a pigeon's egg. Its consistence was moderate; and it was seized and perforated with facility. On the 5th, when it was again attempted, it could not be found, and the meeting was abortive. On the 18th, when the third attempt was made to introduce the instrument, to find and secure the concretion, to grate it and reduce a large part of it was the business of some moments only. Small grains of gravel and much fine sand like cutler's powder were discharged with the urine and injected fluids. By the long forceps of Hunter three or four parcels of viscid mucus enveloping calcareous grains were extracted; and the complete cure was regarded as very near.

These cases demonstrate the facility and the practicability of the lithotritic method in males under ordinary circumstances; and with those in which both M. Civiale and others have since successfully operated, they must be held to be good and sufficient evidence of the general utility of the method as a substitute for the formidable and too often uncertain operation of cystotomy. In females the lithotritic method will be still more easy than men, were not the method by dilatation of the urethra almost equally practicable and advantageous.

It must not, nevertheless, be denied, that cases of calculus in the bladder may be attended with circumstances which render the method of lithotrity inadmissible, and even impossible, while that of cystotomy may be indicated. Thus, if the stone is very large, it may exceed the grasp of such an instrument as can be introduced through the urethra. When the concretion is contained either entirely or partially in a cyst, or in a sac of the bladder, the use of the lithotritic method will be equally unavailing. *Lastly*, concretions deposited on metallic substances, for instance a needle or bodkin, a leaden bullet, or the point of a sword, as has sometimes happened, a button, a tooth pick, or an ear-pick of gold, ivory, or bone, &c. cannot be conveniently destroyed in this mode. If a concretion were known to be deposited on a bodkin or a needle, it might be so far destroyed that the nucleus could be seized longitudinally, and extracted by the urethra. This, however, implies a concurrence of circumstances so singular, that it is almost impossible to imagine the mere possibility. In the case of a concretion formed on a musket bullet, or the point of a broken sword, it is manifest that the operation is quite indispensable.

Before closing this article it is requisite shortly to introduce to our readers the work in which the elaborate essay of M. Tavernier appears. Its title, which is given at the beginning of these observations, sufficiently explains its nature and object. It consists always of three, and occasionally of four parts.

The first contains reviews and accounts of the contents of valuable publications hitherto chiefly foreign, extracts of ingenious essays which have appeared in the German and Italian journals, and occasionally abstracts of essays from the best English journals. It is impossible to speak too highly of the selection of these articles, and the manner in which they have been hitherto performed. They concentrate a great deal of most useful and curious information from some of the best German journals, and other foreign publications.

Thus the number now before us contains the following analyses, reviews, and extracts :—A sketch of the new Italian Doctrine by M. Simon ; Rossi's account of the communication of the lymphatics and veins, from the *Annali Universali di Medicina ;* the second part of a review of Professor Lippi's work on the lymphatics; a note on stenocardia, (*angina pectoris*,) from the *Repertorio di Medicina* of Turin ; account of a curious essay by Dr Urban on the hydrophobic virus, from Hufeland and Osann's Journal ; a review of Professor Dzondi's work on the treatment of syphilis ; the observations of Dr Graefe of Berlin on the use of Zittmann's decoction in the treatment of *Caries ;* (Graefe and Walther's Journal ;) extracts from the observations of Dr Caspari on the utility of hydrocyanic acid (Rust's Magazine ;) a good literary sketch of the history of extirpation of the womb, by Dr Charles Augustus Hesse, (Algemeine Medizin Annalen, 1826.) The second part of the experimental inquiry of Dr Woehler on the passage of substances into the urine, (Journal of Physiology, by Tiedemann, Gmelin, and Treviranus ;) extracts from a paper containing researches on the chemical nature of sweat, by Doctor Anselmino, from the same Journal ; a history of the variolous and varioloid affections which prevailed at Philadelphia in 1823 and 1824, by Drs Mitchell and Bell ; and pathological and therapeutic remarks on yellow fever, by Dr Monges of Philadelphia, both from the North American Medical Journal ; and, lastly, a short account of the work of M. Dutrochet on the immediate agent of vital motion.

The second part is dedicated to the reception of original monographical essays on select subjects in pathology, medicine, and surgery. In the present number the only essay of this nature is that of M. Tavernier on the lithotritic method already noticed. It communicates an elaborate, comprehensive, and very judicious account of the historical progress and present state of knowledge on this operation, and it further illustrates the nature and use of the necessary instruments by five lithographic engravings.

The third part is the *Repertory*, in which is given in alphabetical order, closely printed in double pages, short notices of all points, either new or important, in the anatomical, physiological, and medical sciences. These notices are selected chiefly from the different medical journals published in various parts of the world, and seem to give a fair view of the progress of discovery and improvement.

The fourth part, entitled *Necrological Annals*, is devoted to the reception of short biographical sketches of persons illustrious in medicine and surgery recently dead. The necrological report of No. 2. contains a sketch of the life and writings of the late M. Cullerier of the Bicetre, distinguished for his experience in syphilitic disorders, and a notice of the life, works, and character of Pinel.

In short, this journal promises to be in every respect a very useful book. It well deserves the patronage of the reading part of the profession; and we sincerely trust that it will not fail to attain it.

Art. IV.—*Transactions of the Medical and Physical Society of Calcutta*, Vol. II. pp. 430. 8vo. Calcutta, 1826.

THE second volume of these Transactions fully supports the high character we gave of the first, and its appearance at so short an interval is a proof that the professional zeal and activity of our countrymen are not abated by the heat of the climate. This volume contains twenty-six papers and eighteen notices in the appendix.

From a country so extensive, and varying so much in climate, food, and races of people, as our Asiatic colonies, every communication on the subject of medical topography, or reports on the state of health of districts, is an addition to our stock of knowledge. Of this description we have several valuable papers; the half yearly report of the medical practice at Quilon, by Mr Macauly; on the climate and diseases of Bencoolen, by Dr Tytler; on the endemic fever of Arracan and its medical topography, by Mr Grierson; medical topography of Ramghur, Nagpore, Sirgooja, and Sumbhulpore, by Mr Breton; and medical topography of Aurungabad, by Mr Young.

At Quilon the prevailing diseases were hepatitis and dysentery, originating in dissipation and consequent exposure to the sun.

'Bencoolen has long been noted for the unhealthiness of its climate, but Dr Tytler found it otherwise; for during five months the mortality was not more than at any settlement of equal magnitude on the continent of India, and no native died in hospital of pure fever. The diseases which he found most formidable were of a singular nature, and Dr Tytler, as might have been expected, ascribes them to unwholesome rice. One was gangrenous ulcers depending upon a diseased state of the bone; the other consisted in the production of large horny scales on various parts of the body, which repeatedly fall off, and are reproduced as large as before, in a short time. It is called by the natives *Namby*. Dr Tytler thinks, that, as the inhabitants of Sumatra use rice more sparingly than those of Bengal, " those dreadful bowel complaints which prevail on the continent are unknown in Sumatra, where the evil effects attending the unwholesomeness of grain become developed in gangrene and virulent cutaneous disorders." Whatever may be thought of this hypothesis, the practice derived from it seemed to be useful. It consisted in allowing the patients a more generous diet, with animal food.

The fever of Arracan, so destructive during the Birman war, " does not differ in any material respect from the intermittents of other countries."—" If hepatic and splenic affections are frequent, they are also so in other fevers, particularly in the endemic of Bengal." One peculiarity, however, was observed, an unexpected and almost sudden sinking or failure of the principle of life among convalescents. Mr Grierson thinks that the mortality was greatly increased by deficiency of hospitals and the scarcity of medical officers. The labour of these gentlemen seems to have been immense.

" Almost every surgeon had charge of two corps, some had more; and if we reckon 700 patients as the average charge of each we fall rather short of the truth. To examine each of these twice a-day, or oftener, if necessary; to keep a regular diary of cases; and with an establishment of servants so reduced by sickness, to preserve that correct degree of order and cleanliness required by the regulations, and generally observed in hospitals, will appear a labour scarcely within the compass of one man's exertions. But when we take into consideration the state of the weather, the separate visits to officers in their own quarters, on the sides or summits of hills not to be ascended on horseback, the various circumstances regarding the state of hospitals, but above all, the voluminous accounts, registers, rolls, and returns, to be furnished daily, weekly, and monthly, without the assistance of a writer, the task becomes, both morally and physically, impracticable. The latter occupation alone is abundant employment for one individual; and when required at the hands of the surgeon, cannot but impede, even in common cases, the discharge of his professional duties." P. 217.

Mr Breton's paper and the appendix to it contain much information on the natural history, especially of the animals of the country he describes.

In Mr Young's medical topography of Aurungabad we learn that the troops enjoy very good health from the judicious positions of their cantonments. Of all the diseases met with at this station none is so calamitous in its consequences as rheumatism. Mercurial fumigations and setons near the joints constituted the most approved practice. The natives are subject to all kinds of cutaneous complaints. In critical eruptions they resort to blood-letting, and with success. A case of beriberi, (acute dropsy,) fatal in four days, occurred in a young woman, a nurse in an European family. It was ascribed to the sudden change from low to high diet.

Mr Mackinnon's meteorological register kept at Keitah contains some remarks on the diseases of the place and season.

The three first papers in the volume are upon a singular fever which prevailed epidemically at Calcutta and Berhampore, by Mr Twining, Mr Cavell, and Dr Mouat. The fever prevailed in Calcutta in June, July, and August 1824; at Berhampore it was observed in April, May, and June 1825, and it gradually advanced up the river, being confined to the places on its banks. It was equally remarkable for its scarcely sparing an individual, native or European, within its range; for the shortness of its duration, the severity of the sufferings of the patient; and its scarcely ever proving fatal. Great difference of opinion prevailed as to the nature of this fever. One considered it rheumatic fever, a second scarlatina, and a third synocha; but it seems to have differed from all of these. We select Mr Cavell's description on account of its shortness.

"The symptoms which ushered in, and were characteristic of the various forms of this fever, were pretty nearly the same (except in the appearance of the skin,) whatever was the age or disposition of the patient. The predominating signs were a chilliness extending over the whole frame, quickly followed by a pain and weariness in the limbs, and a general sensation of stiffness and soreness, with a heaviness over the eyes, so excessive as to render the effort to open them painfully oppressive, and a headach, which I believe to be perfectly indescribable. The conjunctiva was extraordinarily red, and suffused in a manner peculiarly characteristic of the disease. The skin was hot and dry, and not unfrequently covered with an almost uniform blush: in some cases indeed it exhibited patches of an inflammatory appearance; in others a papillary, and in one or two instances a vesicular eruption; but very often little more than the flush attendant upon febrile excitement. The pulse was full and strong, and greatly accelerated; the respiration hurried, and generally there was a disposition to vomit. The bowels were usually costive, the urine scanty and high-coloured; while the patient felt frequent alternations of heat and cold, though the impression upon a bystander was, that he was experiencing uniform heat. The tongue in some cases maintained its natural appearance, while in others it was covered with a thick dirty white fur, always moist, although there was a general complaint of thirst, and with but one exception a disagreeably bitter taste in the mouth. These symptoms were succeeded by a considerable prostration of strength for many days." Pp. 33, 34.

Its causes are doubtful, but it was not ascribed to contagion. It did not bear venesection, which increased the debility, and rendered the convalescence slow. Purgatives also required caution in their administration, but an emetic seemed of service.

Mr Wilson has given an account of the native practice in cholera. In desperate cases recourse is uniformly had to the exhibition of animal or vegetable poison. The animal poison is that of a species of Cobra, given in pills. The vegetable employed is a dry root called *Bish*, said to belong to the *Aconitum ferox*, and used for poisoning arrows, and which the Gorkhas look upon as one of the defences of their country, by enabling them to poison the waters on the route by which invaders could advance.

Mr Grierson has described a singular complaint, which he thinks peculiar to the natives of India, and which resists the use of every remedy tried. We think we have seen one exquisite case of it, and many slighter ones in this country.

"It occurs very often after, or together with fever and bowel complaints; but is also found totally unconnected, to all appearance, with any constitutional or organic disease whatever. It exists in various degrees of severity, from an uneasy harassing sensation of heat and tingling, to the painful extreme of burning, destructive of sleep and appetite, in the first instance, and latterly of serious injury to the general health. The sensation of heat is often experienced at the same time in the palms of the hands; and when severe in the feet occasions an aching also along the tibiæ as far as the knee. There is no inflammation, tension, discoloration, or visible change in the limb, the excruciating burning pain being the only symptom present; and the spot principally referred to as its seat is the extremity of the foot, the heel and instep being less affected." Pp. 75, 76.

There are no less than seven communications and notices on the subject of hydrophobia, which show how frequent that disease is in India. The description of its appearances in dogs by Dr Mellis is peculiarly interesting.

"Much as we have reason to regret the absence of such premonitory symptoms as might lead us to be on our guard at times, there are to those who have seen much of madness in the canine race certain indications of the disease which are seldom to be mistaken. The temper and habits of the animal become, as I have before said, remarkably changed. There is either an increased and unusual degree of fondness and vivacity shown, accompanied by something like wildness, or a heaviness and dulness about the countenance, expressive of great anxiety and depression. To these may be added restlessness, redness and discharge from the eyes, dryness of the nose and tongue, loss of appetite, thirst, costiveness, frequent starting, extreme watchfulness, occasional dosing and stupor, difficulty of breathing, or rather irregularity and uneasiness, at times, in respiration, a whining or mournful noise, often followed by a sharp shrill bark, or a dismal howl, trembling of the whole body, as if attacked with the cold stage of fever, foaming at the mouth, with occasional fits of coughing and choaking from viscid saliva, a staggering gait, and paralytic affection of the hinder parts, and occasionally a desire to have them and the belly covered with any thing soft or warm.

"With some dogs there is much confusion of thought, if I may so term it, or an absence of that power of discrimination which the animal before possessed. The senses too are much affected, that of hearing becoming irritably acute, the slightest noise causing terror and distress; while those of smell and taste become so blunted apparently that the animal will often lap its own urine, drink the most dirty and stinking fluids, and eat all sorts of filth, not excepting its own excrement. Dissection has shown this, and it is a never-erring criterion of the disease. The tendency to snap at, bite or worry animals, is not more remarkable in some instances than the uncertain, irritable, and treacherous disposition is in others: and although much has been said about the different kind of madness, viz. the *raging* and the *dumb*, being indicated by, and dependant upon, the temper or disposition of the dog, there is as little foundation for the fact as there is cause for the distinction. The symptoms of each will be found variously united in different animals, and in different states of body; and in a great measure may be accounted for from the nature of the organs affected." Pp. 97, 99.

Mr Breton has narrated some experiments to ascertain the comparative virulence of three species of serpents, and Dr Butter has made us acquainted with the mode of treatment, which he generally found successful.

"My first care was to apply two turns of a stout cord round the limb, immediately above the wound, so as to arrest the circulation through the superficial veins. A drachm of laudanum and an ounce of brandy were then administered in two or three ounces of water, warm when procurable, with a little sugar and essence of peppermint: and this dose was repeated according to the urgency of the symptoms, till the returning circulation and heat of surface indicated a favourable change, which was, I think, in all cases accelerated by making the patient walk about, supported by two men.

"This was the most essential part of the treatment; and in a few instances two or three such doses cured the patient. But in the more severe and numerous class of cases additional measures became necessary to recal the vital energy. The patient was exposed to a large fire, and his throat, chest, and extremities were rubbed with laudanum, ammonia, and sulphuric ether, while frequent attempts were made to make him swallow the usual dose; and when he recovered in a slight degree the use of his legs he was walked or dragged about, and repeated doses of the laudanum and brandy administered, till every unfavourable symptom disappeared. The ligature was then removed. An ounce or two of Epsom salts prevented any injurious effect from the strong stimulants, in whatever quantity they had been used; and the patient, if a Sipahee, returned to his duty, so that none of these cases have appeared in my official reports." Pp. 221, 222.

The very rare occurrence of the rupture of the pulmonary artery is described by Mr Adam Junior. It took place in a native of Switzerland, whose constitution was worn out by intemperance and a thirty years' residence in India. The patient died suddenly, though for two days before he had suffered from paroxysms of acute pain in the right hypochondrium, accompanied with difficulty of breathing, so as to render a change from the recumbent position intolerable. But during two years he had had an intermitting pulse, palpitation, and difficulty of breathing. On dissection the right cavity of the chest was found to be filled with blood. The walls of the right ventricle were very thin, the valves of the artery were in a state of ossification, its parietes extremely thin, even dilated to four times their usual size, its circumference being not less than seven inches. It contained an organized polypus, extending from the semilunar valves about four inches along the artery. The rupture had taken place in the middle, and on farther examination it appeared that there was an opening in the centre of the polypus, through which the blood seemed to be transmitted from the ventricle to the lungs. The aorta and its branches were highly ossified.

Mr Thomas describes a watery tumour or cyst in the liver, and Dr Browne a fatal case of nasal polypus. Dr Gibson relates a case of paraplegia greatly relieved by an accidental burn, confirming the observations of M. Laennec; Mr Baker a singular absorption of the bones of the cranium; Mr Spilsbury an unusual contraction of the eyelids in three individuals of a family; and Mr Bell a malignant disease of the arm cured by amputation.

One of the most curious and elaborate papers in the volume contains an account of the native method of couching by Mr

Breton, illustrated by plates. The instruments employed are an English lancet, with which the sclerotic is punctured; and the couching needle, made of copper, of a cylindrical form, of the thickness of a crow's quill, and about five inches in length, tapering very gradually from the end to within half an inch of the point. The form of this half-inch is three-sided, the apex being blunt. Both the lancet and couching needle are guarded by thread wrapt round them to prevent them from penetrating too deep. The Indian oculists have absolutely no knowledge of the anatomy of the eye, and operate quite empirically. Their success is however remarkable, exceeding that of the most skilful European operators. Mr Breton was so satisfied of the safety attending the use of the Hindoo instruments that he resolved on teaching the art practically to the native students of the medical institution under his charge, and the result has been most promising.

A curious account is given of the Indian penance of Gulwugty by Dr Kennedy, and a practical conclusion drawn from it, that setons may be introduced with ease and perfect safety, and probably with advantage, in a bolder manner than we are in the practice of using them. In this penance the devotee, suspended by two hooks through the skin of his back, is swung round at the end of a lever with great rapidity for a period of from two minutes to seven and a half. In introducing the hooks the only care was to avoid a flesh wound. To effect this the patient was laid on the ground and his back violently rubbed with abundance of oil. This being dried off with sand, another friction equally violent took place with soap shavings. This being again dried with sand, a man, sitting on the patient's shoulders, commenced with his heels a process of kneading, jerking, and working the integuments over the loins, so as to loosen or slacken them, and with great success. When this was done, or rather in the intervals of this process, the operator continued gathering up by little and little a fold of the integuments in his left hand; and when he had mastered as much as he could, with his utmost exertion, force up, he then shoved his hook, not unlike the flesh hooks of the London markets, slowly and deliberately through it, always directing the point outwards. The other hook was now speedily introduced, the operation of fixing both generally taking from three to five minutes. The patient was then hung up, and on being afterwards taken down the curative process consisted in squeezing and sucking out the blood, and afterwards applying a dry poultice of cow-dung and turmeric. In no instance was pus formed or inflammation of any consequence induced.

There is a physiological paper on single vision, and on the

the union of the optic nerves by Mr Twining, in which he combats, chiefly from pathological observations, the opinion lately advanced by Mr Wollaston.

There are several contributions on Materia Medica. Mr Young has confirmed the efficacy of the sulphate of quinine in curing the intermittent fever of Hindostan. General Hardwick has given a notice of the employment by the natives of the roots of the *Cyperus rotundus* in cases of supposed cholera. Six or seven of the fresh tubers were bruised down with as many grains of black pepper, and given to the patient in a wine glass of plain water. In six or seven minutes the stomach became quiet, and the irritable state of the bowels ceased. Great thirst is generally occasioned by this medicine, and the patient is allowed an infusion of cloves and cardamoms for drink. A pretended antidote against the bite of poisonous snakes was found to be the *Phlomis esculenta.* The juice was employed;—snuffed up the nostrils. It is likewise applied externally in headach from cold. The tops are also used as a pot herb. Its taste is merely herbaceous, with a slight aromatic scent. Mr Playfair and Dr Vos relate cases of the successful employment of madar in a case of lumbricus, and another of leprosy. An extract from the records of the Asiatic Society is given, containing an account, by the late Dr Hunter, of a very active vegetable poison, *Meet'ha zuhur*, or *Meet'ha bikh*, sweet poison. It is the root of the *Aconitum ferox*, already mentioned in the native treatment of cholera. Its taste is sweetish, succeeded by acrimony. "Nine grains of it cut into small pieces were given by Dr Tytler to a young cat. In a few minutes a most profuse salivation was excited, and the animal died in four hours violently convulsed. The stomach was found emptied of every thing except the poison, somewhat inflated, but without any appearance of inflammation." Dr Wallich has discovered a new species of Daphne, the bark of which possesses a considerable degree of pungency, and probably may be used as a substitute for the mezereon of Europe. We extract the following notice of the employment of the *Papeeta*, or *Faba Indica*, communicated by Mr Barwell, chief magistrate of Calcutta.

> "Paveeta is generally known among the natives by the vulgate appellation of *Colombo Papita:* it is brought from Manilla also. The dose is prepared by one wine-glass full of cold water, with which the paveeta is grinded on a slab, and administered in liquid. The quantity grinded from the pavita seed is according to the severity of the attack, (without altering the quantity of water.) For instance, in a slight case, an eighth of this seed (herewith sent) is an ample dose; and if the case is a very severe one, one-half of it: thus have I found it an infallible cure in many cases, and have saved

many lives. I must remark here, that the use of this medicine with hot water would be fatal, and a rank poison; consequently it should never be used with hot water, which I am always particular in mentioning, that there may be no mistake.

"Persons attacked with cholera, and resorting to this remedy, should abstain from taking any nourishment whatever, but a dose taken every hour, until the symptoms disappear entirely. To alleviate thirst, cold water may be drank."

The only papers, which remain to be noticed, contain an account of Chinese medical opinion and practice in paralysis, and the abstract of the contents of a work on Chinese medicine by Dr Pearson, both of which are exceedingly curious, but do not admit of abridgement.

We are happy to be able to mention that the third volume is already in a considerable state of progress.

ART. V.—*Versuch einer kritischen Geschichte der bei Vaccinirten beobachteten Menschenblattern, nebst Untersuchungen über die Natur, die Ursachen, und die Verhütung dieser Krankheit.*—Von Dr A. F. Lüders, &c.

A Critical and Historical Essay on Small-pox, as it occurs after Vaccination, with an inquiry into its Nature, Causes, and Prevention. By Dr ADOLPHUS FREDERICK LUDERS, Physikus of Eckenförde in the Duchy of Silesia, Fellow of the Royal Medical Society of Copenhagen. Altona, 1824. pp. 226, 8vo.

2. *De Varioloidibus et Varicellis.* Scripsit NIC. CHRISTIANUS MÖHL, &c.

On Chicken-pox and the Varioloid diseases. By N. C. MÖHL, Joint Physikus of Copenhagen, and Fellow of the Royal Medical Society of that city. Copenhagen, 1827. pp. 111, 12mo.

THERE are now, we should apprehend, few British practitioners, whose doubts are not completely dispelled as to most of the questions regarding the antivariolous effects of vaccination, which have been made the subject of general controversy during the last ten years. But it is only of late that the opinions embraced in Britain have been generally received on the continent, particularly in Germany. Notwithstanding the conclusiveness and authenticity of the facts which were presented to the public in this country at an early period of the varioloid epidemic, and the readiness with which, in the course of two or three years, almost the whole profession in Britain were in consequence prevailed on to abandon several of their favourite doctrines and deep-rooted prepossessions, our continental brethren considered the reform too abrupt and too important to be transplanted among them, so long as it was recommended by the authority of foreign experience alone; and they have hesitated to follow our example, till its necessity has at length been forced upon them, by the breaking out of a similar varioloid disease within the sphere of their own observation. Dr Lüders has very ingenuously confessed his own and his countrymen's obstinacy in this respect. A varioloid epidemic, however, having broken out in various parts of the north of Europe in 1822 and subsequently, they have had an opportunity of subjecting to examination the statements originally made in this country, and first of all in Scotland; and the result has been, that the opinions of almost all physicians of eminence in Germany and Denmark now coincide with our own. We shall presently find from the works of Dr Lüders and Dr Möhl, that this examination has not been conducted without several interesting additions having been made to our knowledge.

The authors are placed in circumstances singularly favourable for investigating accurately and completely the subject of variolous contagion in all its relations. Living in countries where the efforts of the respective governments to diffuse vaccination have long been indefatigable, and have in consequence been attended with extraordinary success, they have had an opportunity of witnessing the introduction and extension of small-pox in districts where it had long been almost or altogether unknown; and they have also enjoyed the rare and important advantage of each holding the office of *Physikus* or Police-physician for an extensive population, in virtue of which office they have charge of all matters which concern the public health of their district, and in particular are brought directly in contact with every contagious epidemic in all its ramifications. The topics to which their attention has been chiefly turned are—the conditions under which the varioloid disease makes its appearance,—the proposition advanced in 1818 by our townsman Dr Thomson respecting the identity of small-pox and chicken-pox and the varioloid disease—and the characters of the true chicken-pox, by which it is distinguished from the varicelloid form of modified small-pox. It is on account chiefly of the two last topics, that we propose to turn the reader's attention to the works of Dr Lüders and Dr Möhl,—the questions involved in them being still made the subject of dispute by the followers of Dr Thomson's opinions, and the experience of the authors having supplied the medical world with some additional information, which will go far to satisfy all unprejudiced minds where the truth lies.

We shall in the first instance, however, take some notice of what they have said on the causes of the varioloid disease and the conditions under which it originates, because their observations afford proofs on an extensive scale of the accuracy of the doctrines now generally received in Britain regarding the relation in which it stands to small-pox, to vaccination, and to inoculation.

On this subject Dr Lüders has adopted many of the erroneous notions entertained throughout this country when the varioloid epidemic was first announced by the practitioners of Scotland in 1817, 1818. He maintains that the varioloid disease *never occurs after regular vaccination*; and in support of this statement he mentions, that among 223,939 vaccinated between 1801 and 1822 in Holstein, where the measures taken by the government ensure a perfect vaccination, there had occurred down to 1824 only two cases of secondary small-pox; and that in Denmark among 447,605 vaccinated, only one such case had been met with. He then goes on to state, that the causes of the frequency of the varioloid disease in other countries, and especially in Britain, have been, first, the formation of too few vaccine pustules; secondly, when there was only one pustule, the opening of it accidentally by friction or intentionally to withdraw lymph; thirdly, the employment of vaccine matter of inferior virtue, calculated to produce only a modified vaccine vesicle; fourthly, modification of the vaccine vesicle by constitutional causes, such as cachexia depending on scrofula, syphilis and the like, or convalescence from a violent or exhausting disease; fifthly, modification of the vaccine vesicle by the coexistence of constitutional cutaneous diseases; sixthly, modification arising from external applications, such as camphor, and mercurial or saturnine ointments; seventhly, modification arising from the co-operation of internal remedies, such as mercury, salpetre, or a course of laxatives. Under the third of these heads, on the employment of inferior vaccine virus, he states as causes of its inferiority, first, its having been taken from a vesicle on the teat of the cow not really vaccine in its nature; secondly, from the modified vesicle produced in those formerly vaccinated; thirdly, from the modified vesicle of those who have previously had small-pox naturally or by inoculation; fourthly, from the modified vesicle frequently produced even by good matter in the less impressible constitutions of adults; fifthly, from the modified vesicle in cachectic habits even among children; sixthly, from the modified vesicle of persons who have at the same time had some cutaneous disease; seventhly, from vesicles which had been some time previously broken; eighthly, from vesicles of too old standing; ninthly, from a dropsical vesicle; tenthly, its having been kept too long after removal from the vesicle; eleventhly, its having been spoiled, though recent, by damp, acid fumes, strong-scented vapours, &c.; and in the last place, its gradual degeneration in consequence of being transmitted by the reproduction of the vaccine vesicle in a great number of individuals successively. He winds up this enumeration by attributing the frequency of secondary small-pox in Britain to the effect of the above-mentioned causes in producing an imperfect vaccination, which, he thinks, must often escape the notice of the profession on account of the careless way in which the progress of the vesicle is watched even at the various public vaccine institutions.

Many of the foregoing statements of Dr Lüders were some time ago advanced in Britain by the believers in the absolute antivariolous power of vaccination, in order to account for its occasional failure, but have by subsequent experience been proved to be erroneous to the satisfaction of every physician of note in the country. It may appear almost unnecessary therefore, for the sake of the English reader, to put Dr Lüders right. But, although the truth has been pretty satisfactorily established, it may not be amiss to notice shortly the results of Dr Möhl's inquiries, which are very pointed and conclusive, and in accordance with all the leading views adopted in Britain.

In the first instance he supplies us with a contradiction to the superior immunity from the varioloid disease, claimed for the inhabitants of Denmark by Dr Lüders, in consequence of the strenuous measures taken by the government to ensure the propagation of a perfect cow-pock. If it is true, that only one case of variola occurred among the vaccinated in Denmark, between the years 1801 and 1822, this extraordinary immunity must have been owing, not to any superiority in the method of vaccinating, but to more accidental causes, and most probably to the want of a variolous epidemic of sufficient malignity. For Dr Möhl, *physikus* of Copenhagen, states, that in the three years preceding 1827, 659 cases came under his care in that city. Such being the fact, it may be anticipated that Dr Möhl does not attach much importance to the effect of the various causes of a modified vaccination, in engendering the varioloid disease. Accordingly, he assures us he has seen as many attacked among those who had the cicatrices of four or six vesicles, as among those who had one only; that he has never been able to discover any facts in favour of the notion, that opening the vesicle lessens its preservative power; and that there is as little ground for believing that the vaccine matter has in the course of time degenerated by frequent transmission in the human subject.

On the last important point Dr Lüders affirms, that the vesicles which are formed by matter procured by successive reproduction from the original virus which was imported from England, have a less areola, and excite less constitutional irritation than those formed by virus from the cow after only three or four reproductions in man; and that he now meets with modified vaccine vesicles more frequently, and large, unequivocal, genuine vesicles more rarely, than when he began to vaccinate. But Dr Möhl states distinctly, that in Copenhagen the vesicles continue to correspond exactly with the original description of Jenner, and that in the late epidemic of that metropolis those recently vaccinated were protected not less, but rather more effectually than those who had been vaccinated ten or twenty years ago. But more of this presently.

Dr Möhl's account of the circumstances under which the varioloid disease occurs is much more accurate than that of Dr Lüders. He observes, that it occurs even among those who have had neither small-pox nor cow-pox; for of 158 cases of small-pox in the unprotected seventeen had it in its modified form:—That it occurs after small-pox, whether natural or inoculated, and that in such cases it is often a dangerous disease, 31 out of 153 having died:—That it does not occur either in them or in the vaccinated, unless a particular epidemic constitution co-operates:—That it sometimes attacks those who have been vaccinated in the most regular and perfect manner:—That the varioloid disease is not materially modified by vaccination, unless the areola of the vaccine vesicle is fully formed before the variolous eruption breaks out:—That the modification is greater and the disease milder if vaccination has been performed a considerable time before:—But that the period within which this rule applies is not unlimited; for *after the first two or three years the varioloid disease is both more frequent, and more severe, the greater the interval which has elapsed since vaccination.*

This last position deserves particular notice, being one upon which the opinions of practitioners are still undecided, and which points at a very essential addition to the present method of practising vaccination. Dr Thomson maintains, as the result of his experience, that the modifying or preventing powers of vaccination are not weakened by time, (*Account of the Varioloid Epidemic of Edinburgh.*) Dr Lüders is of the same way of thinking; for he asserts,—not, however, from personal experience,—that modified small-pox occurs between three weeks and twenty years after vaccination, without the disease appearing to be rendered either milder or more severe by the intervention of a long interval, (p. 103.) But the observations of Adams, Gregory, Cross, Elsasser, Crowfoote, Oswald, and others, have tended to show that the disease is both more frequent and more violent, the longer the interval which has elapsed after vaccination; and the evidence of Dr Möhl is very pointedly to the same purport. Of 653 cases of modified small-pox among the vaccinated, none were under three years of age, only fourteen under five, 102 between five and ten, 173 between ten and fifteen, 187 between fifteen and twenty, 156 between twenty and twenty-five, and twenty-one above twenty-five. As the circumstances under which vaccination has been long practised in Copenhagen render it probable, that a very great proportion of those who have had the cow-pox, have had it in infancy, the foregoing facts supply very pointed proofs of a gradual wearing out of its preservative power. Such is the conclusion at which Dr Möhl necessarily arrives. "I confess," says he, "I am led from these facts to the opinion that the power of vaccination over the human system is in many persons diminished by time; —in many, I say, but not in all, for I have known several persons who were vaccinated twenty years ago and upwards, and who have been repeatedly exposed to the contagion of small-pox without catching it. I can by no means therefore agree with the opinion of Dr Brown of Musselburgh, who lays it down as a general law, that vaccination never excludes the variolous infection longer than five or six years. For the disposition to variola is not the same in all men; so that a remedy which in one will take away this disposition, will in another extinguish it for ever."

We shall now proceed to consider what light the researches of Dr Lüders and Dr Möhl have thrown upon the opinion advanced in 1808 by Dr Thomson respecting the identity of the contagion of small-pox, chicken-pox, and the varioloid disease.

Dr Möhl is mistaken in supposing that Dr Thomson's opinion has become the doctrine of the profession in general throughout Britain. On the contrary, it has never been very generally received, and, if we mistake not, has fewer adherents now than it once had. But it is still believed by too many, and is altogether too important in its consequences to be disregarded, when the attention is recalled so forcibly to it as by the works now before us. In considering the subject we shall first recapitulate the leading evidence brought forward by Dr Thomson, and by the most formidable of his opponents, the reviewer in this Journal of his work on the varioloid epidemic of Edinburgh, (*April* 1820.)

The leading arguments of Dr Thomson are first, that all the cases he had seen of varicella occurred at the same time and in direct connexion with small-pox, sometimes appearing to originate in it, sometimes to produce it;—secondly, that he had never witnessed chicken-pox in those whose disposition to variola had been extinguished by an attack of the varioloid disease; —and thirdly, that chicken-pox is very rare among those who have not been vaccinated. To these apparently strong arguments the reviewer answers, that Dr Thomson disregarded the true characters of chicken-pox as determined by the latest and best authors, and confounded with it the vesicular form of the varioloid disease, which, however, may in most instances be distinguished by a faithful observer:—that doubtless the two diseases sometimes alter their characters so as to approach very near one another, and more rarely alter them so much as to be perhaps undistinguishable:—but, that when the term chicken-pox is restricted to the unequivocal and most frequent variety of it described by Mr Bryce, then it will be found, first, that by natural infection chicken-pox never gives rise to any thing else but chicken-pox; secondly, that by inoculation it never causes the varioloid disease or small-pox; thirdly, that when it is traced ramifying throughout a family or a district, it reproduces itself in the same form and with the same mildness equally in the inoculated, the vaccinated, and the unprotected; and fourthly, that it reproduces itself as often in its mild form among the unprotected as among the protected even when it prevails so much as to be accounted epidemical, whereas all the facts hitherto collected show that when the true varioloid disease prevails epidemically, its form in the unprotected is very often peculiarly malignant.

Returning now to the treatises of Dr Möhl and Dr Lüders, we find the latter states, in reference to the first argument of the reviewer, that he himself, and ten eminent physicians among his acquaintance in various parts of Germany which had been visited by the varioloid epidemic, never saw true chicken-pox engender small-pox. On the second argument as to the effects of inoculation with varicellar lymph, neither of our authors has brought forward any farther facts. On the third argument, its uniformity and mildness in the vaccinated, the inoculated, and the unprotected, Dr Möhl remarks, that he has seen "chicken-pox always put on the same form in the unvaccinated, as in the vaccinated, which would be extremely unlikely to happen, if both originated in variolous contagion (64);" and Dr Lüders observes, that he has often seen "chicken-pox attack equally those who had gone through small-pox, cow-pox, and *chicken-pox*, as well as those who had not had any of these eruptive diseases,

—and without the symptoms being rendered either milder or more severe by the previous disease," (*page* 120.) The fourth argument of the reviewer is of course also confirmed by the statements now quoted.

But our authors have likewise added some new arguments, and one of them of very great importance, as Dr Thomson in his Account of the Varioloid Epidemic of Edinburgh has ingenuously admitted that the establishing it would go far to overthrow the doctrine he was endeavouring to set up.

Fifthly, then, Dr Möhl says he has never seen chicken-pox in families where small-pox prevailed at the same time, or recently before; that he has twice or thrice, indeed, seen in such circumstances an eruption resembling chicken-pox, but never a disease corresponding exactly with its characters as they will be laid down presently (p. 65.) Dr Lüders, on the other hand, alleges he has seen chicken-pox produced by the variolous contagion. He is therefore inclined to adopt a modification of Dr Thomson's views, suggested in 1820 by Lichtenstädt of Breslau,* that varicella was originally produced as an offshoot from variola, gradually weakened by being transmitted and modified through many successive generations; that once produced, it has continued to exist as an independent disease, propagating itself, and incapable of propagating small-pox; and that it may still again arise directly, as in the instances he witnessed, in the manner in which it was originally engendered (p. 125.) The only facts quoted in support of this doctrine, or rather conjecture, are two solitary instances of the common propagation of variola, varioloid disease, and alleged varicella, after exposure to small-pox contagion. One of these cases appears at first sight exceedingly strong: A member of the family had been exposed to the contagion of unmodified variola; and as soon as the existence of the disease was known, namely, on the ninth day, the family was shut up in their house, and the house, which was situate at a distance from any other, was guarded by a sentinel, and access to it was farther rendered very difficult, in consequence of the roads having been blown up by a snow-storm. The introduction of any other contagion, therefore, besides that of small-pox, was extremely improbable. The apparent force of this fact, however, vanishes at once, when we attend to the description he gives of the eruption, in the case which he relates as one of chicken-pox. The eruption was preceded by fever of three days' duration ; it assumed at first the papular form, and it seems not to have become vesicular till the third day after it appeared. We shall presently find that this does not by any means correspond with the description of an unequivocal case of chicken-pox.

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* *Hufeland's Journal, Juni* 1820.

Sixthly and lastly, Both Dr Möhl and Dr Lüders have supplied Dr Thomson with materials for the *experimentum crucis*, which he has himself declared would constitute a criterion, and the sole criterion, of the fallaciousness of his views. " I do not think," says Dr Thomson, speaking of his hypothesis, " I do not think it can be well set aside, till it shall be proved, that chicken-pox occur generally in persons who have not had small-pox or cow-pock, and prevail epidemically without cases of small-pox occurring among them ; but of this I find no unequivocal example in the past records of medicine." (*See this Journal*, xiv. 525.) It is no wonder that the records of medicine should have supplied no such example, seeing how imperfectly chicken-pox was till of late distinguished, and still more how seldom till lately a district of country could be said to be without small-pox. But the political condition of Prussia and Denmark has enabled both of our authors to present Dr Thomson with examples of the most unequivocal nature. " From the year 1809," says Dr Möhl, " till 1823, there was absolutely no small-pox in this city, while during that period chicken-pox was observed every year ; and on that account there is not a Copenhagen physician who entertains any doubt of the specific difference between the two diseases. Betwixt November 1823 and March 1825, while small-pox raged in Copenhagen, chicken-pox still prevailed sporadically, but without our having ever seen them either arise from variolous contagion, or produce variola. When again the small-pox ceased during the five summer months of 1825, chicken-pox nevertheless continued to occur frequently. Next year, when the small-pox epidemic returned, I had frequent occasion to see chicken-pox, but still always under circumstances, which more and more convinced me, that it originated in a peculiar contagion, quite distinct from small-pox." [page 62.] So also Dr Lüders says—" I have observed it every year in abundance, without small-pox having shown itself in any part of the country for a long period either before or after ; and it attacked indiscriminately those who had passed through small-pox, cow-pox, chicken-pox, as well as those who had not gone through any of these disorders, yet without being rendered milder or more severe by the previous disease," [p. 120.]

Taking then into account all the five foregoing arguments,—their direct force, as well as the contradiction they give of the statements on the opposite side of the question, we cannot see any reason for doubting that chicken-pox is specifically distinct from small-pox and its modifications. To Dr Thomson, however, will still remain the merit of having pointed out the resemblance which the two diseases, by mutual departure from their stricter characters, may bear to one another,—a resemblance so close sometimes, that the most experienced will confound them; and of having consequently turned the attention of physicians to the necessity of a more accurate diagnosis between them.

The observation now made leads us in the last place to consider how the diagnosis between small-pox and chicken-pox is to be drawn, and what additional means have been contributed to that end by Dr Lüders and Dr Möhl. Whoever has attended to the account given of varicella by Mr Bryce and Dr Abercrombie will perceive that most previous authors had included under that designation some varieties of eruptive disorders, which it is impossible to distinguish from the common forms of modified small-pox. And although many, or rather most cases of the kind may be proved to have been cases of the varioloid disease, it is at least highly probable that some of them have been cases of chicken-pox, but in one or other of its irregular forms, to the occurrence of which it is liable as well as every other exanthematic disorder. In defining the disease, however, the leading place must be assigned to its most frequent and regular form; and it is obviously to this form alone that we must confine all observations on its origin, contagious nature and the like, otherwise it is impossible ever to arrive at any certain conclusions.

A great deal of attention has been paid to this subject by Mr Bryce, Dr Abercrombie, and the reviewer of Dr Thomson's work. And the result has been, that, in opposition to the opinion of Dr Thomson regarding the impossibility of distinguishing chicken-pox from small-pox, or of embodying in words the idea currently entertained of a pure case of the former disease, we are now in possession of a minute and faithful delineation, which no one can be at a loss to apply in practice. Summing up what had been previously written by Mr Bryce and Dr Abercrombie, and adding his own observations, the reviewer states that the proper unmodified chicken-pox is distinguished, first, by the eruptive fever being generally slight, whereas that of modified small-pox is generally sharp, and of several days duration; secondly, by the eruption being vesicular from the beginning or at least from an early period of the first day,—not papular, as the vesicular form of the varioloid disease always is for a day or more; thirdly, by the absence of a tubercular basis when the vesicles are fully formed,—the vesicles of chicken-pox being hardly accompanied with any swelling around them, while those of modified small-pox are in the first instance elevated on solid tubercular bases; fourthly, by the great thinness and fragility of the cuticle covering the vesicles. In applying these characters, it is added, two precautions must be observed: On the one hand the eruption must be seen as early as the second or third day, because at a later period the chicken-pox eruption sometimes acquires a tubercular base, and the varioloid eruption loses it; and on the other hand the judgment must be directed by the general eruption, not by the appearance of a few vesicles differing from the generality.

To these characters others of some consequence have been added by Dr Möhl and Dr Lüders. Dr Lüders says there is a difference in the seat of the eruption. The varioloid eruption is formed in the true skin, as is shown by the hard, elevated base, which remains after the lymph is removed by puncture and pressure, [*page* 111.] Chicken-pox on the other hand, is situate in the cellular tissue between the skin and cuticle. This may be perceived, as Mr Bryce also formerly pointed out, by opening a vesicle, and examining its edge after the lymph has run out; no excavation or elevation will be perceived, but a surface level with the surrounding skin, [*pages* 111 *and* 134.] Notwithstanding the clear and simple diagnosis which this description supplies, it is quite plain that Dr Luders has repeatedly, as in the instance formerly mentioned, described under the title of varicella cases really of the varioloid disease. Dr Möhl agrees with Mr Bryce and Dr Abercrombie, as to the rapidity with which chicken-pox assumes its proper vesicular structure. He had never been fortunate enough, he says, to see it on the first day; but on the second day he has uniformly found it vesicular. He farther adds another character, not always present, however, namely, itchiness of the eruption. And he has given a minute description of the crusts, which he says are characteristic, being irregular, uneven, opake, of a pale brownish or yellowish colour, formed of the lymph and collapsed cuticle, and falling off, as Dr Monro pointed out, not in a single piece like the crusts of variola, but in small fragments.

Dr Möhl has added some judicious observations on other forms of eruption admitted as varieties of varicella by Dr Willan, and his own countryman Heim. While he considers that most of the cases of the kind were actually examples of modified small-pox, he seems ready to admit that varicella does sometimes put on a form hardly distinguishable from that disease. Of this hardly a doubt can be ascertained.

We cannot quit this analysis without warmly recommending to the reader both the works we have been noticing. That of Dr Möhl particularly is perhaps the best epitome that has yet appeared on the subject of small-pox and chicken-pox.

ART. VI.—*Memoirs of West Indian Fever; constituting Brief Notices regarding the Treatment, Origin, and Nature of the Disease commonly called Yellow Fever.* By JOHN WILSON, M. D. R. N. London, 1827. 8vo, pp. 217.

WE must say, that from this small and not very pretending production we have derived some gratification. It is neither the most learned nor the most comprehensive account of West Indian fever that has yet appeared. It will by no means bear comparison with the clear and pithy delineation of Dr John Hunter, the interesting accounts of Dr James Clark and of Thomas Clark, the elaborate descriptions of Rush and Bancroft, or the minute and faithful painting of Jackson. But it is distinguished for views, which, if not entirely original, are at least developed with great ingenuity; it is pervaded with a strong vein of sound judgment and good common sense; and it breathes a spirit of independence in reasoning and thinking, mitigated, nevertheless, with a tone of becoming calmness and moderation, which renders it a work of no ordinary value.

It is unnecessary to give much or even any attention to the first memoir, which is devoted to the description of the phenomena of West Indian fever. We conceive we do injustice neither to the present nor to any other author who has written on this subject during the last ten years, when we say, that, after the descriptions given by the authors mentioned above, novelty, or even improved views of the mere history of this disease, are totally out of the question. When we have said, therefore, that the description of Dr Wilson may be an accurate record of the phenomena of the individual epidemics which he witnessed, and that he gives proof of understanding both the nature of the disease and the most efficacious mode of treatment, we have said all that we conceive the subject requires; and we forthwith quit it entirely.

The second and third memoirs, in which the author considers the merit of the several opinions which have prevailed, or are now prevailing, on the cause of endemic fever, will form a more legitimate subject of discussion; and to them chiefly, with some passing observations on the matters treated in the fourth and fifth memoirs, which conclude the treatise, we at present propose to direct the attention of our readers.

In the second memoir, Dr Wilson proceeds at once to the consideration of the prevailing opinions regarding the causes to which West Indian fever has been attributed, under the four heads of atmospheric heat,—contagion,—marsh miasmata,—and the principle of vegetation.

The alleged influence of the first agent is very quickly disposed of, and perhaps scarcely required serious refutation. Dr Wilson justly remarks, that the circumstance of yellow fever never being seen in situations, for instance Kingston in Canada, and Moscow and various places in Russia, in which the atmospheric temperature is equally high during summer as in places noted for the prevalence of that disease, affords a satisfactory proof that the cause assumed is insufficient to produce the effect.

The consideration of the influence of the second agent, to which West India fever has been attributed, constitutes a much more difficult task. Amidst the confused and indistinct ideas which prevail on the nature and operation of contagion, it is not easy to select the point of commencement in the investigation of its truth ; and it is difficult to obtain, regarding an existence so shadowy and unsubstantial, those palpable arguments which the rational physician requires for the foundation of his belief.

According to the present tone of medical opinion among those who contend for the existence of contagion as a cause of West Indian fever, it is understood to be so in one of two modes. According to one, yellow fever is absolutely, essentially, and at all times the result of contagion. It differs entirely from endemic fever, never proceeds from it, and never passes into it. According to the other view, the same causes which produce endemic fever may, by the superaddition of a contagious property generated in the subjects of the disease, give rise to another form, propagated only by contagion.

The advocates of the former doctrine adopt the ordinary view of contagion when held as an inseparable adjunct of any disease. They imagine that the contagious cause of yellow fever is at all times diffused through the atmosphere in the situations in which this disease is indigenous ; that it has seasons of inactivity during which it does not affect the persons of men, not from loss of power, but from absence of the circumstances which are supposed requisite for its operation. Dr Wilson justly remarks, that the defenders of this hypothesis have never pointed out these circumstances, nor shown why contagion is dormant at one time and active at another, in the case of yellow fever epidemics. No connection has been traced between these periods of action and the states of the atmosphere as to heat, humidity, or other meteoric conditions, or between the condition of the soil or the peculiarities incidental to individuals. It is manifest that, to infer the operation of contagion under such circumstances, is a mere *petitio principii*, equivalent to saying that yellow fever is a contagious disease because it springs from contagion. "We get weary of distinctions," says Dr Wilson, " where we cannot find difference, especially when we are told to estimate the difference by our *knowledge*, that the one is a contagious and the other not a contagious disease."

More generally prevalent, especially among those who have considered the origin and propagation of yellow fever out of the West Indies, is the doctrine of contingent contagion, or that which, admitting the disease in sporadic cases to be the offspring of the physical peculiarities of the country, regards it when epidemic as endowed with a contagious property. This doctrine, though specious and of easy application, Dr Wilson considers as not less erroneous. The circumstances which are supposed to communicate the contagious property, crowding, filthiness, and insufficient ventilation, are nowhere to be found in any of the situations in which yellow fever is most prevalent in the West Indies. Neither of these circumstances are to be found in ships of war, in barracks, or in encampments, in all of which cleanliness is more scrupulously observed than in the dwellings of the citizen ; nothing like crowding has occurred since the peace establishment ; and ventilation is perfect, and even to excess, in the opinion of some, especially in the barracks. Yet, notwithstanding the absence of these generating causes, fever is equally often epidemic as during the war, and as many die in proportion to the number employed. Under similar circumstances, Dr Wilson concludes that the doctrine which invests yellow fever, as an offspring of the soil, with contagious properties, derived from contingent circumstances in the persons of those exposed to it, is inadmissible.

Another difficulty with which he justly remarks this hypothesis is embarrassed, arises from uncertainty as to the time when, and the manner in which the fever undergoes the change from a non-contagious to a contagious disease. It is impossible to conceive the change to be effected by extent of prevalence, rapidity of progress, severity of symptoms, degree of mortality, or by any circumstance within the cognizance of the senses. If, therefore, argues Dr Wilson, the character and effects of the disease which, according to the hypothesis, is originally without contagious property, possess nothing which can either generate or communicate this property, or can indicate that it is communicated, how can its origin or existence be demonstrated ? The only evidence, in short, on which the existence of a contagious property is inferred, is the negative one derived from the difficulty of accounting for the progressive propagation of the disease on any other principle.

But even this negative evidence, Dr Wilson contends, has never been adduced. " Let a single authenticated case of the disease appear in the neighbourhood of another person under its influence, and where a local cause for its appearance cannot be found, and the conviction of contagion must follow. But while the earth on which we tread, and the habitations in which we dwell, contain the ample and palpable means of producing the disease, we are unwilling to admit the operation of an agent so far beyond our comprehension as contagion. In ships there are abundant materials for the production of West Indian Fever, as I shall show in the sequel; and all over the West Indies and adjacent shores, it is every now and then springing up and disappearing, in ways which preclude the suspicion of contagion. It frequently appears in the United States, and sometimes in the south of Europe, in circumstances which satisfy every one that it does not arise from contagion, excepting those who maintain that it arises from contagion alone; and with them I have no argument."

In the phenomena of the appearance and propagation of yellow fever there is a class of facts which have ever afforded the advocates of contagion a plausible ground of argument, and which to those who demand proper evidence for the presence of contagion has often proved the source of much perplexity. First, a vessel with a healthy crew arrives at a harbour; and shortly after, for instance a few days or a week, yellow fever appears among the inhabitants, and among the crew at the same time. Secondly, a vessel arrives with a sickly crew at a port, suppose Nassau in New Providence; and at the time of her arrival the same fever which prevails among her crew is thinning the garrison of Nassau. In the first case, the fever is ascribed to the presence of the stranger ship, which is supposed to have imported contagion, or a contagious cause of disease. In the second case, it is impossible to ascribe the disease to the stranger ship, because it was prevailing before her appearance. Yet, in the former case, Dr Wilson contends there is quite as little reason for ascribing the presence of the fever to the arrival of the vessel as in the latter. In both cases the season for the usual appearance of that disease had arrived; and the other circumstances to be found either in the state of the ship, in the persons of its crew, or in the physical surface of the port, and the persons of its inhabitants, whether civil or military, were only wanting to develope the febrile epidemic. To these circumstances the author afterwards directs the attention of the reader very fully.

To vessels landing with sickly crews on healthy islands, among the inhabitants of which the fever afterwards does not appear at all, does not even affect the surgical and other patients of the hospital to which the sickly men are confined, or appears shortly after and prevails extensively, he applies the same views. Under such circumstances, Dr Wilson contends, it is equally impossible, as in the previous case, to say that fever would not have arisen among the inhabitants, though the sickly vessel had never appeared or never touched the shore. He concludes, in short, that in the case of vessels landing crews, whether in a sickly or in a healthy state, it is quite a gratuitous assumption to imagine,—that it is requisite to look to the vessel for the origin of the disease, the cause of which is at all times found in the physical circumstances of the islands themselves. The whole point is very closely and conclusively argued, and merits the special attention of candid and unbiassed inquirers.

The conclusion to which Dr Wilson then arrives regarding one island he applies to all, and to all places in similar physical circumstances. In alluding briefly to the case of the ship Bann and the island of Ascension, which has been generally supposed conclusive on this question, he declines considering it at length from imperfect knowledge of the circumstances. He observes, however, that even this case presents a difficulty to the hypothesis of absolute or even contingent contagion, in the fact that the disease did not appear at Great Mountain, though the intercourse between that post and the sick of the Bann was equally open as between the sick and the other posts where it did appear; and though a man belonging to that post had been sometime on board the vessel. Another difficulty in this case consists in the fact, that the same fever had appeared in the island of Ascension in the year 1818; and as this must have been the product of the soil, where are the reasons which should lead the other to be regarded as proceeding from a different source?

On another topic closely connected with this, Dr Wilson expresses his sentiments with firmness and spirit, yet, upon the whole in a tone so temperate, as to give his opinions the greater weight. This is in reference to the strong, and rather dictatorial language which Sir Gilbert Blane has on all occasions, but more especially in his Elements of Medical Logic, used in treating the question of the contagious nature of yellow fever. We profess, as every one must, the highest regard for Sir Gilbert Blane, as a learned and intelligent physician, and one whose writings in general have contributed much to the improvement of medicine, and especially the medical police of our navy. If he had never written any thing more than his work on the diseases of seamen, and his essays on the origin of ague, he would even be entitled to a high rank in public estimation as a judicious and patriotic physician. In addition to these intrinsic and essential virtues, we shall not forget that the learned baronet is aged, and therefore venerable, and that his character commands respect even from those who differ from him in modes of thinking. And if we concur with the opinion expressed by the author before us, we hope Sir Gilbert has more good nature, if he thinks there is not room for the exercise of liberality, than ascribe it to any other feeling than that of wishing to see a doubtful subject investigated dispassionately, and the truth established upon substantial grounds, without any colouring from prejudice, from party, or from the authority of names.

It is perhaps a subject of regret that the learned author of the elements introduced into that work the question of the contagious character of yellow fever at all. As it stands it is a perfect example of special pleading, and not a favourable one, we humbly think, of medical logic. In short, one who reads the work with the expectation of seeing unfolded the principles of reasoning to be observed in conducting medical inquiries, is almost irresistibly led to the inference, that the work was written for the discussion on yellow fever, and not that the latter was introduced to illustrate the principles developed in the former. Though undertaken as if to be investigated, the question is almost prejudged. It is doubtless under this feeling that not only the present author, but many others, conceive their reasonings and inquiries on a subject on which it is impossible to be certain, to be treated not merely with harshness, but with want of candour. That one observer should differ from another in the explanation of the cause of a fact, the origin of which is admitted to be obscure, is no reason why he should be accused of unsound reasoning, or should have his powers of observation and correct statement called in question. All unbiassed persons, interested in the discovery of truth only, must agree that, by such a course, the progress of science, instead of being accelerated, is most certainly retarded.

Having vindicated himself and those who think in the same manner, Dr Wilson terminates his general arguments against the contagious origin of yellow fever, by stating, what he conceives to be a legitimate conclusion, that in all places where tropical heat of equal intensity is acting on a soil containing the materials of fever, spontaneous fever may be expected. If, on the contrary, it could ever be proved that this disease is carried beyond a certain atmospheric circle, and there arises in persons who have not been within that circle, he would admit it to possess contagious properties, either native or acquired, because it would then be impossible to account for it otherwise. Thus, if a ship should be distinctly proved to have conveyed West Indian fever from Havanna to Halifax, and that it was spreading among the inhabitants of the latter place; or if it should be proved, that, in consequence of a person labouring under the disease being conveyed from Vera Cruz to Xalapa, fever arises in the person of one individual at the latter; or if it has ever been seen to be carried from the plain of Liguanea to the persons of those who reside in the elevated parts of the mountain (3500 feet above the level of the sea,) where it has never been yet seen; he would not deny the competency of such evidence to establish the contagious origin of the disease. He confirms the observation made by Humboldt, that though yellow fever is an annual and fatal visitant at Vera Cruz, it is never found at Xalapa; and that a line of demarcation may be traced as distinct as that which separates the respective vegetable products of these places.

These general reasonings are corroborated by the particular facts of the epidemic yellow fever which appeared on board the Rattlesnake at Port-Royal in August and September 1824. These circumstances, for the detail of which we refer to the work, exemplify the statements above made, that when a vessel which has put to sea with a few cases of fever is obliged, from the subsequent increase of sickness, to make for a harbour, the sick men may be conveyed to the hospital, and die or recover, without the disease appearing in any of the surgical or other patients, or among the inhabitants of the place. While the disease attacked the people on board in rapid succession, it was not propagated in a single instance beyond the sides of the ship.

The reasoning of Dr Wilson against the contagious origin of West Indian fever is chiefly derived from negative evidence; and to those who do not reflect, that, with the exception of the facts last mentioned, it is difficult to obtain any other, this reasoning may appear not so perspicuous and easily followed as might be expected. This can by no means be said of that part of the memoir which shows the insufficiency of marsh miasmata. For against this his arguments are direct, pointed, and altogether conclusive.

After defining the term marsh, and restricting the epithet *miasmata* to signify the gaseous products of this sort of soil exclusively, he proceeds at once to overturn the hypothesis of their generating fever, such as the West Indian, by adducing evidence of two kinds. By the first, he undertakes to prove that the disease in question frequently prevails where no cause of this kind can be in operation; by the second, that marshes abound where yellow fever rarely if ever occurs.

1*st*, From the testimony of Dr Wilson, which we believe is corroborated by that of many other competent observers, fever may originate and prevail on board ships in the West Indies with all the symptoms of yellow fever, yet without the possibility of tracing it to any thing like marsh either in the vessels themselves, or in the places adjoining to where they are moored.

Few ships continue eighteen months on the Jamaica station without a visitation of West Indian fever, in different degrees of severity, but generally in such a form as to merit the name of epidemic. It was at one time imagined, and Bancroft laboured hard to establish the fact, that the moisture which is always present in the holds of ships, and mixed with vegetable matter undergoing in this climate rapid decomposition, is quite equivalent to a marsh on shore. Some plausibility for this view might be derived from the gravel and sand which was then in use for ballast. Since the introduction of pig-iron bars, however, for this purpose, and the use of iron tanks for the water, no argument of any strength whatever can be derived from this source. The hold of a ship, especially in the public service, is as clean and dry as a floor. Yet yellow fever, with its usual malignity, has not been wanting.

In two vessels only during the last twelve years, the Pyramus and Childers, in which yellow fever was extensive and severe, were any extraneous matters found which could give a colour to this hypothesis. Yet to these vessels, admitting the influence of this matter, the fever was not confined. It was equally prevalent, and not much less malignant, in the other vessels enumerated by Dr Wilson, in which nothing of this kind was recognised. With regard to two of these, the Iphigenia and Rattlesnake, in which the fever was equally well marked, equally severe, and equally mortal as usual, Dr Wilson speaks with the confidence derived from public and express inquiry in the former case, and from personal observation in the latter. Of all the ships, therefore, he infers, which suffer epidemic attacks of this disease, an extremely small proportion, perhaps one in a hundred, may be found to contain any accumulation of materials resembling a marsh; and it hence results that the disease must arise from something else than marsh miasmata in ships.

Of Barbadoes the most southern of the Caribbean chain of islands, (north lat. 13°,) the physical characters are such as to preclude entirely and everywhere the idea of marshy surface. The formation is calcareous, not very compact, the soil clayey, the surface rather flat, but dry; and, as it has been the longest cultivated of our West Indian possessions, by trenching, draining, and cleansing, it has undergone all those improvements which remove a territory as far as possible from the state of mere nature. The surface is, in short, dry and sound to an extreme; and with the exception of a small muddy spot at the bottom of the carenage in Bridgetown, there is nothing on the whole island like a marsh.

Corresponding with these peculiarities in physical character, is the fact that none of the modifications of ague are endemic in Barbadoes. Agues, indeed, are here seen in the persons of those who have derived them from some other place of their production; and those whose constitution has permanently imbibed the disease, may, as in other places, relapse in consequence of debauch or adequate exposure to cold. But so far is it from being the native offspring of the soil, as in other places, that, under ordinary circumstances, speedy recovery from ague may be confidently expected in Barbadoes.

Yet this island, so dry, and so free both from the cause of ague and the disease itself, has been at all times, and still continues to be occasionally afflicted with fever in its most malignant and mortal forms. During the whole of the French revolutionary war, it occasioned annually a considerable mortality, which underwent little diminution till its termination. The limited number of garrisons employed during the peace establishment has rendered the loss comparatively smaller. But the epidemic of 1817, the mortality of which was great compared even with that of the war years, proved an exception. These facts, Dr Wilson remarks, render it impossible to establish the connection of cause and effect between marshy exhalations and West Indian fever. The former do not exist in Barbadoes. The latter has been abundant even of late.

Inferences of the same description Dr Wilson derives from the physical characters of two noted yellow fever posts in the West Indies, Brimstone Hill in St Christopher's, and Stoney Hill in Jamaica.

The former, though a bare dry volcanic rock, utterly incapable of affording any thing similar to marsh exhalation, has been more than once the scene of severe and fatal yellow fever, with all its usual symptoms. The instance of 1811 and 1812 was so pointed, that it has been generally advanced as a proof of yellow fever from contagion. It was never shown, however, from what source, or by what channel that contagion was conveyed to Brimstone Hill barracks.

Stoney Hill, in like manner, which rises above the plain of Liguanea in Jamaica, is a mass of calcareous rock, dry, sterile, and sprinkled so thinly with soil, that it is difficult to perceive whence the trees with which it is covered derive nourishment Yet the troops stationed in the barracks here have repeatedly suffered severely from yellow fever; and the 77th regiment, which, on its arrival from England in 1825, was stationed here, was almost literally destroyed by this epidemic.

2d, In proceeding to the second species of argument, that marshes occur, nay, may abound, where yellow fever is rare, or completely unknown, Dr Wilson first contrasts the city of Rio

Janeiro with that of Vera Cruz; both under nearly equal parallels of latitude; both built on a low sea-shore ground, skirted by high mountains, though at unequal distances; the religion of the inhabitants the same, and their habits and modes of life very similar. Yet, while Vera Cruz, without a rood of marshy soil, is a hot-bed of West Indian fever in its most violent forms, within Rio Janeiro, though placed near an extensive swamp, and liable to ague, yellow fever was never known.

The instance of the settlement of Demerara is still more appropriate. This narrow tract, low, level, extending along the sea coast, and intersected with canals and ditches, alternately inundated, and covered with stagnating oozy water, abounding with vegetable matter, where the soil is a swamp, and the ditches are marshes, might be supposed to be the prolific source of yellow fever. The general suffrage of medical opinion, indeed, influenced by theoretical considerations, pronounces this territory unhealthy; and the inhabitants are doubtless subject to agues and remittents. These diseases, however, are not severe; and Dr Wilson states, from official documents, that during six successive years, from 1805 to 1812, the proportion of deaths to the number of the sick was much below that of any other British settlement in the West; as low indeed as in England during the same period. He further informs us, that the inhabitants are healthy, and long-lived on an average of years, in a degree little inferior to those of Great Britain. What is most to the point, however, is that, as far as he has been able to ascertain from published and oral statements, the concentrated West Indian fever is exceedingly rare in Demerara.

The argument is closed by the example of Honduras. The physical situation and character of this colonial tenure are well known. Close on the sea, and occupying part of a large bog, on which foundations for the buildings have been gradually formed, principally from the ballast of ships which have gone thither to load mahogany and dye-woods, it is represented by Dr Wilson, who was there in 1825, to be during the rainy season a perfect lake, where teal and snipe are shot from the windows, and where the mosquitoes, sand-flies and bull-frogs contend incessantly to prevent sleep during the night; yet so parched during the dry weather, that the inhabitants have difficulty in procuring water before its termination. With Porto Bello to the east, nevertheless, and Vera Cruz to the west, Honduras is not unhealthy; and though the inhabitants have much ague, and some remittent fever, yellow fever is scarcely known among them. An intelligent shipmaster, who had been twelve years in the Honduras trade, told him, that in Honduras he never lost a man from fever, and that he had given up touching at

Jamaica in consequence of his crew invariably suffering at that island from the disease. The same conclusion is confirmed by the fact, that the vessel to which Dr Wilson was attached spent the most unhealthy period of 1825 at Belize in safety.

The principle of vegetation to which yellow fever is ascribed by Jackson, Doughty, and we believe Dr Fergusson, is quickly disposed of. After observing how unintelligible and indefinite this term is, Dr Wilson shows very shortly, that the hypothesis is completely contradicted by the immunity of Demerara and Honduras, in which the process of vegetation is vigorous and rank to an extreme, as already mentioned. Some ingenious and rather interesting observations on the origin of sporadic cases of fever, and the nature and distinctive characters of endemic and idiophatic fevers, conclude the memoir.

The third memoir, in which Dr Wilson proposes certain new opinions regarding the origin of West Indian fever, is entitled at least to the character of being highly interesting. Without undertaking to inquire whether these opinions are altogether new, or to what extent they can be regarded as such, we proceed directly to bring them before the reader. It must not be forgotten, that, in bringing forward these opinions, the author, with becoming modesty, offers them as hints only, " possessing little value in themselves, but which may call forth valuable information from others."

In directing his attention to the circumstances, combined or separate, under which the endemic fever of the West Indies is observed to originate, Dr Wilson thinks it possible to trace a connection between that disease and the geological structure and physical peculiarities of the places in which it is chiefly prevalent. The islands of the Carribbean Archipelago consist chiefly of calcareous matter, of what the geological writers term secondary formation, and sometimes of entire masses of coral. The texture of this is so loose, porous, and traversed with crevices, as to form at all times a never-failing receptacle for every species of decayed and decaying wood, leaves, &c. which Dr Wilson thinks there is good reason for believing the immediate source of the peculiar fever, which has been so long indigenous in this part of the world. Whenever the calcareous subsoil is scantily covered with earth and herbage, fever is frequent and severe; and the thinness or absence of this covering appears to operate in allowing the woody matters to run to decay, in a situation in which the gaseous products of decomposition are incessantly escaping, to affect the frames of those persons who reside in their vicinity.

This view he illustrates, in the first place, by reference to Jamaica, great part of which is a calcareous crust, destitute of superincumbent soil in many places, in which fever is always most frequent and severe. The vicinity of Rock Fort, for example, three miles east of Kingston, the basis of which is coral, and the mountain behind a naked calcareous mass, has been at all times so fatal to our troops, that whole garrisons have been successively destroyed. To the men of the naval force on this station it has been not less pernicious; and to prevent the mortality invariably occasioned by watering parties, the use of floating tanks, wrought by negroes, has been found indispensable. Stoney-Hill, already mentioned as peculiarly fatal to the 77th regiment, consists of a great mass of calcareous rock, intersected by deep fissures, and split in many places into large fragments, piled on each other, and crumbling into powder; situations very favourable, Dr Wilson concludes, for the reception and decay of woody matters.

Fort-Haldane at Port-Maria, which is an elevated mass of limestone, washed on three sides by the sea, might be thought to be the most healthy station in the West Indies. It is, nevertheless, the source of fever so deadly, that for some years it has not been garrisoned. In November 1824, when, by the urgent entreaty of the local authorities, in apprehension of insurrection among the slaves, a detachment of the 50th regiment was stationed in the barracks, the appearance of fever was so early, and its havoc so great, that it was found necessary in six weeks to remove the detachment, after losing one-third of its number, and with nearly the whole remainder labouring under the disease. Dr Wilson thinks there is good reason for regarding the mangroves, which cover the banks of two streams which fall into the bay, to be the material source of this epidemic sickliness. At Port-Antonio, distinguished for similar geological relations, the port and barracks have been abandoned for the same reason. Port-Royal, the extreme sickliness of which is too well known even in this country, is a narrow coralline promontory, covered with sand and gravel only, the public buildings and the dwellings of the inhabitants.

He proceeds to establish, in a very clear and interesting manner, the same connection between the loose, perforated, or fissured calcareous formation, and the prevalence and fatality of yellow fever at Port of Spain in the island of Trinidad; at Fort-Louis in that of Martinique; at Point-a-Pitre, Fort-Louis, and Fort-Fleur de Pays in Guadaloupe; at Port-au-Prince and Fort-Bizoton, St Domingo; and at Bridgetown in Barbadoes. Even Nassau in New Providence, though situate to the north of the northern Tropic, and therefore enjoying the alternation of seasons; yet as presenting, especially at Fort-Charlotte, a dry limestone flat, is consigned to the same class of febriparous situations.

To Carthagena, the principal sea-port of Colombia, the same views apply, if possible more forcibly. Placed in the 10° north latitude, it is exposed to the influence of high temperature; and its seasons are divided, like those of tropical regions, into dry and rainy, not hot and cold. Resting on a calcareous basis, which is in many points destitute of soil, it possesses a source of disease still more unequivocal, in the mangrove forests with which its coasts are abundantly covered.

The observations which Dr Wilson has had an opportunity of making on the geological characters of different places in which West India fever is indigenous, he admits, are too imperfect to justify any general or very positive conclusion. Further, though completely established, it could be regarded as little more than affording the means for the favourable operation of the direct agent of the disease. This material, or, as Dr Wilson terms it, palpable cause of the disease, he undertakes to show, is the gaseous product, not of vegetable or herbaceous matter, but of trees, shrubs, or any sort of woody matter in a state of decomposition. In short, the hypothesis advanced by Dr Wilson is, that yellow fever is of ligneous origin.

Difficult as may appear the task of establishing this position, Dr Wilson has adduced a very strong body of evidence in its favour; and he has placed his different facts and arguments in order so clear and forcible, that the doctrine assumes, on the whole, an imposing and persuasive aspect.

It has ever been a perplexing point, in tracing the circumstances of the history of the rise and progress of several yellow fever epidemics, to account for the cases which appear on shipboard. It is not unfrequently in a vessel newly arrived that the first cases appear. In some instances, they appear while the vessel is at sea. (Thracian and Lively, and many others.) In many instances they appear among the crew of a ship actually lying at quarantine, or at least in such circumstances that she has communication neither with the shore nor with other vessels. In all these cases, the advocates of the hypothesis of contagion had recourse at once to their ordinary principle. But though the operation of contagion was assumed, it was always difficult and often impossible, as the history of various febrile epidemics testifies, to trace the exact source of the contagion.

Those, on the contrary, who contended that yellow fever arose independent of contagion, were embarrassed in not being able to demonstrate the presence or the operation of the cause to which they ascribed the production of yellow fever. It is not a little curious to remark the strained ingenuity, and the farfetched explanations which have been employed to prove that the hold of a ship was truly a marsh, and under certain circumstances emitted marshy exhalations.

It is not more than just to Dr Wilson to say, that he surmounts these difficulties passably well, and with much plausibility. In the Iphigenia, the vessel already alluded to, when stationed at Kingston in January and February 1820, yellow fever appeared in a malignant and fatal form,—having attacked 121 persons, and caused thirty deaths. Neither Dr Bancroft, nor Dr Adolphus, deputy inspector, nor Mr Macnamara, surgeon of the naval hospital, could recognize any of those matters which are supposed to emit marshy exhalations; and the circumstances under which the disease appeared, wholly precluded the idea of contagion. In August and September 1824, the disease appeared in the crew of the Rattlesnake, and prevailed in a severe and fatal form till the 29th October. The vessel was new, and arrived in Jamaica on the 29th June. Infinite attention had been paid to cleanliness and ventilation; no extraneous matter except some chips of wood, mineral tar, and a little water under the limber boards, was found in the hold; and it was a fortnight after these matters were removed, that the first case of the disease appeared.

Admitting then the inference, that yellow fever is generated within the sides of the ship itself, and maintaining at the same time the inadequacy of the causes hitherto assigned to explain the phenomena; he proceeds to show that the wood forming the interior of the holds of ships undergoes in tropical climates a great change, during which some of its constituent principles suffer decomposition, and pass off in a gaseous form. This change is manifested by the wood becoming dark, by its shrinking and becoming denser in structure, at the same time losing weight; in short, by seeming to be partly charred. The extent to which this process is carried, and the nature of its results, Dr Wilson represents to be modified by the previous condition of the wood, by the degree of heat, and probably by the interior arrangements of individual ships. In a vessel newly built, arriving in the West Indies in the hottest season of the year, remaining for weeks in harbour, with the hold cleared and heated by stoves, as was the case with the Rattlesnake, the process is speedily completed. According to Dr Wilson, fever in such circumstances appears early and spreads rapidly; but when it ceases, it returns not again. In a vessel, on the contrary, which arrives in the cool season, which is much at sea, and the hold of which is not dried by stoves, the process of decomposition is slow and imperfect, and may never be completed. In such a vessel fever will never be severe and very fatal, but it will often recur and produce sickliness and death. till the last day of her continuance on the station. These facts are confirmed by the phenomena of fever in many vessels in the West Indies. A good example is given in the case of the Isis.

The nature of this change the author does not venture to specify further, than by saying that it is totally different from that denominated dry-rot, and that the ligneous fibre is not injured, but simply condensed, and partly charred.

Its connection with the appearance and progress of the fever in different parts of the vessel is interesting. Confined at first to a small space, the disease begins generally in the vicinity of the pumps and main hatch-way, where the keel is most dependent, where water draining from other parts is accumulated, and where heat is most intense. Thus the first cases and the greatest mortality take place in the births of the midshipmen and marines, which in frigates are placed on each side of the pumps and main hatchway. Its subsequent progress depends on the trim of the vessel, and the inclination of the keel from the horizontal position; the disease spreading in the most dependent direction. These general inferences the author afterwards confirms by a clear and curious collation of evidence, from the phenomena of fever as it took place in the Rattlesnake, the Isis, the Pylades, the Ferret, the Scylla, and the Lively.

The next point is to find in land situations analogous circumstances for the production of the malady. This the author readily does, in the great quantity of woody matter constantly undergoing decomposition in the West India Islands, but especially in the forests of mangrove bushes, with which the coasts in general, and the banks of streams are profusely covered.

"The mangrove grows, and runs to decay rapidly. The branches, after rising a few feet, bend towards the root on which they ingraft themselves. From the bow thus formed, other branches spring up, which in like manner insert their extremities into different parts of that from which they grow; and so on till an impenetrable dwarf forest is spread over the surface of the water. While the lower part of this water forest is undergoing rapid decomposition, the upper part is in a state of luxuriant growth and beautiful verdure, so that it looks like a piece of basket work supporting a shrubbery. From its structure and habits, the mangrove appears destined to speedy decay, to which surrounding circumstances are highly favourable. In consequence of the rise and fall of the tides, part of it is alternately wet and dry, and the whole is exposed to the influence of high temperature. Hence, whatever deleterious product is furnished by decomposing wood, must be furnished abundantly here."

Another modification of ligneous decomposition, adduced by Dr Wilson as a source of yellow fever, is the mast-ponds or mast-trenches, in which large spars are long buried, with the view of giving them durability. "A few years ago, a party from H. M. S. Euryalus, was employed in raising one of these spars. They had heard so much of danger, that they kept to windward of the log during their labour, believing that some human bones which they turned up, were pregnant with mischief. One of them, however, a midshipman, thinking that such precautions were tokens of unmanly fear, sat down to leeward of the trench, laughed at the apprehensions of the others, and taunted them with the imputation of weakness unworthy of sailors. It was remarkable that of the whole party, that gentleman alone was attacked with West Indian fever next day, of which he died soon after; and that at a time when the disease was not in the ship or in the harbour,"

It is too manifest that the ordinary mode of explaining cases of this description, by referring them to the extreme and long-continued effort necessary in duty so laborious, cannot apply in the present instance. The individual attacked was the only one who had not been working.

In adducing as the third modification of this source of yellow fever, the timber materials of which the docks, wharfs, and jetties of sea-ports in North America, noted for yellow fever epidemics, are constructed, the present author is anticipated, if we mistake not, by Rush and others, who denied the contagious properties of yellow fever. He has the merit, nevertheless, of rendering the theory much more consistent than formerly, by the collateral and analogous evidence which he has adduced.

Our account of the second and third memoirs has been so lengthened, that it is impossible here to enter minutely into the consideration of the two last ones. In the fourth, the author investigates the question, whether it is a peculiar or a common disease. His observation of its phenomena, lead him to maintain that it does not remit, in opposition to the views of Dr John Hunter, Dr Rush, and others. He maintains also the presence of a *peculiar morbid affection of the alimentary organs, and a want of consent between the power of the disease and its external manifestations;* and from these circumstances, he submits the question, whether it is not a disease *sui generis.*

The last memoir consists of remarks on the manner in which the cause of fever, especially that of West Indian fever, impresses the human body. On this point the author, we conceive, appears to least advantage. But in extenuation, it may be said that he is not alone; and the intrinsic difficulties of the subject are so great, that his failure is not wonderful. It is needless, therefore, to continue our observations further.

In conclusion, however, it is our duty to say, that the present work contains by far the clearest and the strongest refutation of the hypothesis of contagion yet adduced; and if the author succeed in establishing by subsequent proofs and illustrations, the doctrines which he so ably maintains, he will be entitled to the merit of giving the most satisfactory explanation of the etiology of yellow fever, that has yet appeared.

ART. VII.—*The Morbid Anatomy of the Human Brain; being Illustrations of the most frequent and important Organic Diseases to which that viscus is subject.* By ROBERT HOOPER, M. D. Bachelor of Physic of the University of Oxford, &c. &c. London, 1826. 4to, pp. 36. Fifteen Engravings.

2. *The Morbid Anatomy of the Brain.* By ALEXANDER MONRO, M. D. Professor of Anatomy and Surgery in the University of Edinburgh, and President of the Royal College of Physicians, &c. &c. Vol. I. *Hydrocephalus.* Illustrated by Copperplates. Edinburgh, 1827. 8vo. pp. 200.

MORBID anatomy is the most certain foundation of pathological knowledge; and the more clearly and forcibly its several facts are demonstrated and impressed, the more valuable and satisfactory will be those doctrines which constitute the science of pathology.

So far as the pathological states of the human brain have yet been investigated, the difficulties of the study have been so great, as to disconcert the most resolute, and weary the most patient inquirers. The causes of this confusion are too obvious to require explanation. Of one, however, the influence is so considerable, that it must have often been matter of sincere regret to all zealous cultivators of this interesting department of pathological anatomy. In certain situations in which we find great, remarkable, and manifold morbid changes, symptoms have been carelessly or imperfectly observed, or, it may be, from the circumstances of the case, are entirely unknown. Conversely, in many situations, in which the train of morbid phenomena, which were supposed to depend on some morbid state of the brain or its membranes, has been observed and registered with the utmost care and the most assiduous perseverance, the disease unexpectedly subsides, and the patient recovers;—or he dies, and, from some peculiar and insurmountable difficulties, an autoptic inspection cannot be obtained.

As a slight remedy to these evils, and as furnishing more precise information than mere description alone can convey, engraved delineations of morbid changes are of the utmost value to the student. Next, indeed, to inspection of the parts, when still recent, and before their colour, consistence, and mutual relations have undergone any change, delineations constitute the most accurate record to which the pathologist can have recourse. We may add that they are of great advantage in rendering the knowledge of the leading facts of pathological anatomy more generally accessible.

To this description of publications, the two works now before us belong; and both are well entitled to the attention of the student of pathology.

The publication of Dr Hooper, which is at present the most complete, is the beginning of a series of coloured engravings, intended to illustrate the most important morbid changes incident to the viscera of the human body. Judging from the specimen afforded by the present volume, the author appears to follow as a model the elegant collection of Dr Baillie.

It consists of fifteen engraved delineations, representing the morbid states and changes most frequently observed in the brain and its membranous coverings. In these delineations, the parts are represented of their natural size, most faithfully and beautifully coloured after the natural appearance of the recent parts, and, we may justly add, with great attention to the rules of art. The work is chiefly designed as a faithful register of valuable objects of morbid anatomy; and the engravings are merely accompanied with such letter-press description as suffices to explain them clearly.

In a brief introduction, notwithstanding, Dr Hooper takes a view of the principal morbid changes in organization, structure, and conformation incident to the brain and its coverings. The general division embraces five branches; 1. inflammation and its effects; 2. tumours; 3. diseased structures and unnatural appearances without tumefaction; 4. morbid collections of fluids secreted between the membranes and in the cavities; and 5, extravasated fluids.

The second head, that of tumours, presents eight subdivisions; a. aneurism; b. *cephaloma*, the cerebriform or encephaloid tumour of foreign authors; c. *chondroma*, cartilaginous tumour; d. *haematoma*, bloody tumour; e. *hygroma*, or encysted serous tumour; f. *melanoma*, or melanosis, black deposit; g. *osteoma*, or bony tumour; and h. the varieties of morbid structure, designated under the general name of *scrofulous tumours*.

Under the head of alterations of structure, and unnatural appearances without tumefaction, Dr Hooper places unnatural flaccidity, and firmness, pulpy softening, and change of colour.

The principal of these morbid changes are exemplified, by delineations of parts arranged in two general divisions; the first seven engravings being confined to the morbid states incident to the membranes.

The first and second engravings present very beautiful specimens of inflammation of the *dura mater* in different stages of progress. In the third and fourth, we find the appearances of the same disease in the *pia mater* and arachnoid membrane delineated. The fifth exhibits the appearances of osseous deposition in the membranes; the sixth tubercles; and the seventh tumours of different degrees of consistence in the dura mater.

The remaining engravings are devoted to the elucidation of the morbid states of the brain and cerebellum.

The eighth engraving presents a very fine view of the effects of inflammation as seen in a transverse section of the cerebral hemisphere; the second figure being a good example of pulpy softening (*ramollissement*) in different stages; and the ninth exhibits different forms of cerebral abscess, in various stages, and of various size. In the tenth plate the observer is presented with a very good view of the bloody tumour (*hœmatoma*) as it occurs in the hemisphere of the brain; and the scrofulous growth as it takes place in the cerebellum is represented in the eleventh.

The twelfth contains views of a variety of important morbid changes. 1st, The white tubercle (*cephaloma*) of the cerebellum; 2d, melanosis in disseminated spots, which Dr Hooper terms the black tubercle; 3d, the vesicular or hydatiform state of the choroid plexus; 4th, tubercle with cartilaginous spots attached to the choroid plexus; 5th bony tumour of the brain; and 6th, encysted tumour (*hygroma*) of the pineal gland.

The thirteenth is a very fine representation of the brain containing an encysted tumour in the middle lobe on the right side; and the fourteenth exhibits a transverse section of a brain containing three vesicular globular cysts, not unlike hydatids. The subject of the fifteenth and last is of a more ordinary character, extravasation of blood from rupture of a vessel in its substance—forming an apoplectic cavity.

It is scarcely possible to do justice to these engravings as works of art. They are highly creditable to the enterprise of Dr Hooper; and we trust he will be encouraged to proceed with equal success in the completion of the useful work which he has now undertaken.

The second work, that of Dr Monro, though in its present form less complete in relation to its avowed subject, is nevertheless more comprehensive in its details; and by the instructive facts which it contains, is an important acquisition to the pathological reader.

The present volume is the first part of a work which Dr Monro proposes to publish on the morbid anatomy of the brain; and we do its author the justice to say, that it promises to be a work of considerable interest and of much value to the pathologist.

Though avowedly on water in the brain, it is not confined to this subject solely. Dr Monro has taken occasion to introduce a good deal of valuable matter on the pathological relations which subsist between serous effusion and various morbid states of the brain and its membranes.

Thus, in the first chapter, in which the effusion of fluid into the ventricles is considered in a general way, he introduces some good observations on the comparative consistence of the brain in the sound condition, and in various forms of disease; and he communicates some interesting cases and remarks on hydatids and cysts of the brain, and especially that constituting the apoplectic cavity. With the view of illustrating the circumstances under which watery effusion not unfrequently takes place, the author gives a view of the organic derangements incident to the membranes, and of the several sorts of tumour most frequent in the brain itself. Under the former head he adverts to inflammation and its effects on the dura mater, ossification, and tubercular growths of that membrane, fungous tumours, and scrofulous growths. Under the latter head, he describes briefly the saponaceous tumour of the brain; the scrofulous, illustrated by three cases; the adipose, or the *lipoma* of foreign anatomists; the vascular-lobulated tumour; the scirrhous, the bloody tumour, encysted bony tumours, and melanosis.

The bloody tumour, or that which the professor describes as of the colour and consistence of clotted blood, and which he thinks bears a near resemblance to fungus hæmatodes, is that which has been mentioned by some foreign authors under the name of *haematoma*, or sometimes *cephalæmatoma*, (Zeller.) Little is known of it, save that in this situation it is invariably fatal. The description is exemplified by the outlines of a case.

The whole is illustrated with a short account of preparations, demonstrating various morbid states and growths of the dura mater, transmitted from Sir James M'Grigor to the author.

The second chapter is subdivided into five sections, the first of which is a history semeiographic, pathological, and therapeutic, of water in the brain. Dr Monro is very unwilling to admit that this effusion is ever the result of inflammation; and has taken considerable pains by collation of facts and arguments from various sources, and experiments by himself, to demonstrate that, instead of being connected with inflammation, it is rather to be imputed to scrofula, or those causes which occasion derangement in the circulation of blood in the brain; and that though inflammation may sometimes precede this effusion, it is by no means essential to it.

This conclusion, it is perhaps unnecessary to controvert; since its validity depends very much on the idea attached to the term inflammation. We entirely agree with the learned author, that every organic disease, every morbid growth, and a large proportion of morbid changes of every kind taking place in the brain, or its membranes, or in both, terminate in the effusion of serous fluid into the ventricles, and also in the subarachnoid cellular tissue. But few facts, we conceive, are better established than this,—that effusion of serous fluid in the ventricles of infants and other young subjects, (we have seen it even at the sixteenth, eighteenth, and twentieth years,) is preceded by a certain degree of accumulation of blood in the minute vessels, if it cannot be termed inflammation, of that part of the *pia mater* named the choroid plexus.

The second section on *hydrocephalus, accompanied by enlargement of the skull*, owes its chief interest to a case given by the author, and another from Sir Everard Home.

The fourth section, on *enlargement of the brain, with little or any fluid within* the ventricles, is illustrated by a valuable case from Dr Duncan Junior.

The fifth section, on *hydrocephalus*, with malformation of the skull, brain, and spine, is illustrated by a clear and accurate account from Dr Christison, of a case in which the co-existence of internal and external hydrocephalus, yet without mutual communication, was distinctly established.

In speaking of the possibility of condensing or compressing the brain into smaller bulk, (p. 166,) a verbal mistake may be noticed in saying the "*exhausted* receiver," instead of the "receiver" simply.

The present volume is illustrated by five engravings of pathological drawings, three of which are coloured after the recent appearance of the objects. These engravings are finished with great neatness and taste; and though, as objects of art, not equal in delicacy to those of Dr Hooper, they are nevertheless entitled to a high degree of praise, for their general truth and fidelity of representation. The fourth plate, exhibiting an instance of tumour of the brain, is a very good specimen of coloured delineation. The fifth one is not less valuable as a plain engraving.

In the appendix, we find in a letter from Dr Kellie, on tubercles and their effects in the textures in which they appear, some notes and remarks which are conspicuous for the accurate observation and sound judgment for which that gentleman is distinguished. A good case of tumour also is communicated by

Dr Abercrombie, and one of tubercles of the *pia mater* and brain, by Mr Watson.

Upon the whole, the merits of the present volume will make the student of pathological anatomy look with just expectations of much instruction from the second, in which the author proposes to illustrate in the same manner the difficult subjects of apoplexy, epilepsy, mania, and the effects of external injury.

ART. VIII.—*Essai sur les Tubercules, &c.—An Essay on Tubercles.* By HENRY CLERMOND LOMBARD, M.D. Member of the Royal Medical Society of Edinburgh, &c. Paris, 1827. 4to, pp. 54.

THE thesis of Dr Lombard suggests some new views on the nature, origin, and progress of tubercles, partly original, partly derived from the oral lectures of Andral. It professes to treat of tubercles generally, but is chiefly confined to the most common of them, those formed in the lungs.

The views most generally entertained of the formation and progress of pulmonary tubercles are those advanced by Laënnec in his work on Mediate Auscultation, and founded by him on the pathological experience of twenty-five years. But M. Lombard undertakes to controvert several of the most positive and important of Laënnec's statements, and we are inclined to think that in some he has succeeded. Laënnec, it will be remembered, maintains that the miliary tubercles or granulations of Bayle are not, as that author conceived, a distinct variety of tubercle, but the commencement or first rudiment of the future white, cheesy tubercle. After a time, according to Laënnec, a whitish or yellowish point begins to form in the centre of the translucent grain, it soon becomes altogether opake, then it begins to soften in the middle, and the softening gradually extends towards the circumference, and increases in degree till the whole mass forms a thick matter, which approaches in nature to pus, and is eliminated by expectoration through an opening in one or more bronchial tubes. This account presumes that the tubercular matter is in the first instance organized, and endowed with vitality. M. Lombard on the other hand, follows Bayle, Chomel, and Andral, in assigning to the miliary granulations an existence independent of tubercles, and indeed does not account them of a tubercular nature at all. According to his dissections they are of two kinds. One, the formation of which has been traced by Andral, is described as the result of *lobulated pneumonia*, and as consisting of hypertrophy of the parietes of the air-vesicles. The other, which has been ascertained by

M. Lombard himself, is a similar production formed in connexion with the blood-vessels which supply tuberculated portions of the lungs.

The proper tubercle, according to M. Lombard, is in every one of its seats—the lungs, the external glands, the spleen, the mucous membranes—a secretion deposited in the cellular tissue. It is a dead substance, incapable of undergoing any internal changes, opake from the beginning, and increasing by the juxta-position of more tubercular matter. A tubercle is sometimes simple, situate in a single mesh of cellular tissue, and not containing within it any of the regular textures in which it is deposited; at other times it is compound, (*T. infiltré* of Laënnec) or consists of several simple tubercles closely applied to each other, and inclosing therefore between them smaller or larger portions of pulmonary tissue. The only mode of elimination which M. Lombard has been able to establish by his pathological researches is by the mass being separated from the surrounding tissue by inflammation and suppuration of a thin stratum of the latter, in consequence of which the tubercle becomes loose, breaks down, is mingled with purulent matter, and then discharged by expectoration. A tubercle never softens in the centre, as Laennec supposed, unless it is a compound one, in which case the organized tissue included in it may inflame and suppurate, breaking down the tubercle within, and so giving it to a careless observer the appearance of a softening or suppuration of the proper tubercular matter. A similar deceptive appearance may arise from the occasional formation of an indurated cyst around a simple tubercle, and likewise by the tubercular matter having been deposited around a bronchial tube, so that there is formed in its centre a little cavity containing muco-purulent matter.

Having thus stated the results of his observations on the origin and progress of pulmonary tubercles, M Lombard next proceeds to consider their causes, predisposing and occasional. The predisposing causes which he enumerates are, 1. Hereditary disposition, the operation of which it seems some French authors, M. Louis in particular, are inclined to dispute; 2. Peculiarity of temperament, engendered by living in cold and moist countries, and in unhealthy situations; 3. Age, children being more liable than adults; 4. Sex, females suffering more than males; 5. changes in the animal fluids caused by insufficient food, by bad food, or by an unfavourable mode of professional life. On these points the author has advanced little that is new. The section on the occasional causes is more important. The author espouses warmly the doctrine lately advocated by Dr Alison of this university, and by Andral,—that a common cause of tubercles is inflammation of the lungs. This account of the origin of tubercles has been very pointedly contradicted by Laennec, who, in reference to some vague conjectures of Broussais, has laboured to prove that they are not connected either with acute or chronic pneumonia, or with catarrh, or with pleurisy. M. Lombard maintains that they are the frequent consequence of catarrh, acute pneumonia, and chronic congestion in the lungs, and considers the tubercular matter as a secretion deposited in particular constitutions in the course of the inflammatory disorder, just as serum is effused in others, and the fibrin of hepatization in others. He rests this opinion on the conjunction remarked by Dr Alison of tubercles and hepatization in persons who had died of pneumonia, and who, previous to the pulmonary affection, had been in good health,—on similar observations made by Andral,—and on others made by himself. We may add that we have lately seen in the Infirmary of this city a striking example of the same description, in a patient who died of inflammation of the right brachial vein, united, as that disease often is, with pneumonia of the same side. The man died eight or ten days after the vein began to inflame, and on inspection the lung of the right side was found partly hepatized, partly infiltered with serum, but in a still greater degree studded with large grayish yellow masses analogous to the matter of tubercles, and of the variety which Laennec has named *tubercules infiltrés.* The left lung was likewise studded with tubercles, but did not exhibit any hepatizations or serous infiltration. Previous disease of the lungs to the extent in which the tubercular matter was effused appeared hardly possible, as the man was remarkably robust in figure, and followed a very active profession, that of a dancing-master, without having ever had any pulmonary affection. Another fact on which the author rests the opinion now referred to is the production of tubercles in animals when the bronchial membrane is mechanically irritated by the injection of mercury into the trachea,—a fact which was ascertained lately by M. Cruveilhier. A considerable part of M. Lombard's thesis is taken up with an investigation of the arguments used by Laennec, Louis, and Rostan against the doctrine of the inflammatory origin of tubercles. In this investigation he has shown much acuteness and considerable success. It is impossible, however, for us to follow him in the details.

But it is not to acute inflammation only that M. Lombard ascribes the origin of tubercles. Passive congestion, he thinks, may have the same effect, upon the principle that whatever obstructs the circulation of a part increases secretion there, and that secretion is in particular habits apt to give place to tubercular deposition.

The remainder of the work is occupied by a short detail of the symptoms of incipient tubercles and of the treatment. On the former of these subjects the author has advanced nothing new: the existence of the disease can be ascertained only presumptively. Under the head of the treatment he insists strongly on a rigorous prophylactic regimen being observed by those who have a tendency to the disease by hereditary taint or constitutional disposition, and has laid down the rules for accomplishing this object succinctly and accurately. While treating of constitutional disposition he very properly takes notice, that as two opposite temperaments are subject to tubercles, viz. the lymphatic and sanguineo-nervous, two very opposite systems of treatment are required in individual cases: in those of a lymphatic temperament the constitution must be strengthened; in those of a sanguineo-nervous temperament an opposite plan must be pursued, particularly in respect to diet. This difference explains, he thinks, the very discrepant accounts given of the success of the two methods of cure which have been applied to the early stages of phthysis. Having considered the prophylaxis he proceeds to the curative treatment, under which he considers the two important questions—relative to the susceptibility of tubercles undergoing absorption, and the possibility of tubercular ulcers healing up. He considers that tubercular depositions may be absorbed, like every other secretion, though this does not appear to occur often in the lungs. He likewise insists that there is nothing in the intrinsic nature of tubercular ulcers to prevent them healing, and that they are prevented doing so only by the formation of new tubercles around,—caused by the inflammation in the vicinity of the ulcers. The fatal issue of most cases of phthysis is owing not to the drain caused from any particular set of tubercular cavities, but to the constant successive formation of new ones. The great object of the curative treatment, then, should be to keep within moderate bounds the inflammation by which the tubercular matter is eliminated; and the principal difficulty is to accomplish this indication of cure, without at the same time interfering with the prophylactic treatment, on which dependence must be put for correcting the tubercular tendency of the constitution.

The author seems to us to have been, so far as his inquiries have gone, very clear and successful in his expositions, and his views and statements cannot fail to attract the attention of all those who are now engaged throughout Europe in the investigation of pulmonary disorders. In some particulars he has certainly gone far to prove that the prevailing doctrines require to be subjected to fresh inquiry.

PART III.

MEDICAL INTELLIGENCE.

PHYSIOLOGY.

On the cause of death after the ligature of the eighth pair of nerves (Zeitschrift für Physiologie. Erstes Heft, 1826.*)*—Professor Mayer of Bonn has investigated this interesting subject experimentally; and he infers that, in almost every instance, unless some of the accidents presently to be mentioned intervene, death is owing to the blood in the heart and lungs losing its power of continuing fluid. If the animal, says he, dies soon after the nerves have been tied or divided, the cavities of the heart, and the arteries and veins of the lungs are found after death filled with a soft coagulum of black blood. But when death does not ensue for forty-eight hours or upwards, then the clots are firm, white, fibrinous, free from colouring matter, and they pervade the pulmonary vessels to their minute ramifications. These concretions, he thinks, are formed during life, and are the cause of death; and they are owing to the blood, in consequence of being withdrawn from the operation of the nervous influence, tending to separate into its proximate principles as it does after death. The value of the author's experiments is very much depreciated in our eyes by his not having stated the period after death within which these polypoid concretions were met with. It is well known that similar concretions are often found in the human subject, and though some pathologists believe they occur during life, nay are even sometimes the immediate cause of death, the more general doctrine is that they are a pseudo-morbid appearance altogether, and do not occur till after death. In order to prove that the concretions observed by Professor Mayer are not, as the phenomena now mentioned are generally conceived to be, the product of operations after death, he ought to have examined the bodies of the animals on which he experimented immediately after death, or rather when death was approaching If fibrinous concretions are found in such circumstances it is unnecessary to point out the light which the observation will throw on many analogous pathological phenomena. In making experiments for ascertaining this point it is important to attend to the accidental causes of death which have been pointed out by the physiologist of Bonn. They are in the first place the passage of the food into the trachea, in consequence of the paralysis of the constrictor muscle of the cardia, and the muscles of the glottis; and secondly, a more rare accident, emphysema of the lungs and thoracic cellular tissue, probably caused by rupture of the bronchial mucous membrane during the violent efforts sometimes made to inspire.

Professor Mayer has farther made some valuable observations on other more subordinate effects which result from ligature or division of the eighth nerves. He agrees with the late experiments of Breschet, Vavasseur, and Edwards, who observed that, contrary to the statements of Wilson Philip, digestion is not essentially impaired. He found that the animal temperature was not diminished by the operation, or at least not nearly in proportion to the diminution of the breathing. But the most remarkable phenomenon he has mentioned, is the invariable and extraordinary discrepancy in the effects of the operation on the breathing and on the action of the heart; while the respirations are diminished in number to a half, a third, or even only a sixth of their natural frequency, the pulsations of the heart are doubled or quadrupled in frequency. The cause of this discrepance he thinks is, that the obstruction caused by the concretions in the pulmonary vessels necessitates an increased and more frequent action of the heart, in order that a sufficient quantity of blood may be transmitted through them.

On the Narcotic Properties of Carbonic Acid Gas.—[*Arch. Gén. de Méd. Juin* 1827.]—Physiologists and toxicologists have long agitated the question, whether the deleterious properties of the carbonic acid gas are owing to its being a positive poison, or a simple asphyxiating gas; and it is important to determine the question, as some material conclusions result in regard to the treatment. The experiments and opinions of Bichat and Nysten had led to a general, but we have long thought an erroneous belief, that it is no more a poison than azote or hydrogen, and that it causes death simply by the exclusion of respirable air. The following experimental arguments contained in a paper by M. Collard de Martigny prove that it is a true narcotic poison.

1. If carbonic acid instead of azote, be mixed with oxigen in the proportion of the constituent gases of atmospheric air, animals cannot breathe it for a minute or two, without being attacked with all the symptoms of narcotic poisoning. If the proportion be even reversed, so that there are seventy-nine parts of oxigen, and only twenty-one of carbonic acid, the effect is similar. Nay, animals will die in a few minutes even in a mixture of atmospheric air and carbonic acid, which will support combustion; and persons have been taken out insensible from pits, where lights left behind them continued to burn. 2. Simple asphyxia, from the deprivation of air or substitution of hydrogen or azote, does not prove fatal to man in less than four or five minutes, to some other warm-blooded animals in less than fourteen minutes, to fishes in less than an hour, to frogs in certain circumstances in less than five days. Now an atmosphere of carbonic acid gas kills the perfect animals in seventy seconds; water impregnated with it often kills fishes in the course of six minutes, and frogs in fifteen or twenty-five minutes; and the latter animals perish in an atmosphere of the gas in even a shorter time. 3. The narcotic symptoms caused by inhaling the gas may be also produced by its application to other surfaces, besides the pulmonary mucous membrane. Persons accustomed to live abstemiously have been intoxicated by drinking mere aërated water. If the body be enclosed in an atmosphere of carbonic acid gas, with due precautions for the maintenance of the breathing, the usual symptoms of poisoning with carbonic acid are produced,—sense of weight in the head, indistinct vision, pains in the temples, ringing in the ears, giddiness, exhaustion, an indefinable feeling of terror; and if this experiment be continued long enough, death will be the consequence, as M. de Martigny proved upon animals. 4. The symptoms caused by immersion in an atmosphere of carbonic acid gas are quite different from those caused by immersion in hydrogen or azote. If an animal is plunged in hydrogen for two minutes, it recovers in a few minutes either spontaneously or with the aid of insufflation of air. But immersion for that length of time in carbonic acid gas either kills it outright, or leaves it under symptoms of narcotic poisoning, which the most vigorous treatment does not always remove till many hours have elapsed. These arguments appear quite decisive of the positively poisonous action of the gas. The paper is closed with some judicious remarks on the treatment of poisoning with carbonic acid. The only novelties are a recommendation of ammonia and other stimulants in the form of injection in preference to any other way of applying them, and a denial of the danger mentioned in our last number as having been remarked by M. Leroy d'Etiolles to be caused by too strong insufflation of air into the lungs. M. de Martigny could never remark in dogs the dangerous effects of brisk insufflation noticed by the latter gentleman; and indeed, M. Leroy has since acknowledged (Journ. de Chim. Méd. iii. 354.) that he was led into error in his experiments,—that the sheep is thus injured, but not the dog.

Experiments on the Effects of Bryonine on the Animal System. [*Nouvelle Bibliothèque Médicale, Mai* 1827.]—M. Collard de Martigny has extended to this principle the investigations of Orfila on the effects of the bryony root, and has obtained results which show that the chief properties of the root reside in the principle in question. The bryonine was first discovered by Brandes and Firnhaber; but M. Collard de Martigny has found it more easily separated by the process more lately suggested by M. Fremy, which consists in saturating the juice with ammonia, filtering the fluid, and evaporating it till a pellicle is formed on the surface. The pellicle is the impure bryonine, which is to be purified by solution in alcohol. Twenty-two grains of this substance killed a rabbit in ten hours when administered by the stomach, no particular symptoms intervened, and after death the stomach was found thin in some places, thick and hard in others, and its villous coat red, and interspersed with a few white granulations. Twenty grains injected into the pleura caused death in seven hours, attended with all the signs of pleurisy, namely, serous effusion, the formation of a pseudo-membrane, and gorging of the lungs. Thirty-four grains thrust under the skin of the neck and back of a dog killed it in fifty-eight hours, causing extensive inflammation and suppuration around the wound. Bryonine therefore is a pure irritant, which does not appear to act through absorption. It is much more powerful than the root which yields it; yet a given quantity is not so active as the quantity of the root from which it is procured.

CHEMISTRY AND MATERIA MEDICA.

Method of recognizing the stains caused by blood on steel instruments.—(*Journal des Progres des Sciences*, T. iv. 1827.) Professor Orfila has recently made some experiments on this subject with the view of illustrating certain questions in Medical Jurisprudence,—in which namely doubts are entertained whether stains on steel instruments are caused by blood, by acid juices, or by rust; and the following are the criterions which he proposes. 1. When the instrument is heated to 80° or 90° F. the stain becomes brighter if it is caused by blood, but is not altered if it is caused by rust or lemon juice. 2. A drop of hydrochloric acid does not alter a blood-stain, but dissolves rust or acid stains. 3. The blood-stain steeped in water parts with its colouring matter to that fluid, and the red colour is retained on filtration, while stains caused by rusting or by vegetable acids either do not tinge water at all, or, if they cause a reddish-brown muddiness, it is removed by filtration. 4. The colouring matter derived from the blood-stain may likewise be recognized unequivocally by its chemical properties, and in particular by the effect of chlorine: A minute quantity of chlorine turns it green, a larger quantity decolorizes it altogether, and an infusion of galls added to the decolorized solution causes a dark-red precipitate, which is the colouring matter in union probably with tannin. 5. Strong nitric acid destroys the colour of the stain caused by blood; the diluted acid dissolves it, forming a red solution, which precipitates red with infusion of galls.

East India Senna.—By far the greatest part of the senna of commerce bears this title, although it is certainly not produced in the East Indies. We are informed by Dr Adams, secretary to the Medical Board of Bengal, to whom we applied for information, that the senna used in India is known in the bazars by the title of Senna Mukkhi or Mekki. It is imported from the Persian Gulf, and is probably the growth of the Arabian shore. From Calcutta it is re-exported to London. A specimen which Dr Adams has had the goodness to send us for comparison differs in no respect from the very sharp-pointed East India senna of the shops. The small quantity of senna which is the produce of India is blunt-pointed and very inferior in quality.

East India Opium.—We have been indebted to the attention of the same gentleman for a specimen of very pure opium, manufactured by an officer on the Bengal establishment, of which the sensible qualities accurately correspond with those of the best Turkey opium, and which is very unlike the tar-like substance hitherto in use in that country. It has also been analyzed by the late Dr Abel, who found the quantity of morphia yielded by it greatly to exceed that from the ordinary kind in India. As it can be procured in any quantity, there appears to be no reason why it should not entirely supersede the use of Turkey opium, not only in our East India settlements, but even in this country.

Lac Boxes.—The opium was sent home in a box manufactured by the same gentleman of lac and wax, which, by being soldered at the joinings, effectually excluded the air and moisture. We understand that common wooden boxes or chests can also be lined with the same material, and rendered perfectly air-tight. They will at the same time preserve their contents from moisture and insects.

New Escharotic. (*Rust's Magazin für die gesammte Heilkunde*, 1826.)—Professor Hanke of Breslaw has proposed the muriate of zinc as an effectual stimulant and escharotic, which possesses all the conveniences and advantages of the nitrate of silver, and is at the same time of moderate price. As a stimulant he has employed it in a state of dilute solution, for almost every form of ulcer in which stimulant applications are found beneficial; as an escharotic he has used it in cancerous ulcers, fungoid excrescences, nævi materni and the like, and likewise for making issues; and he has farther substituted it in place of tartar emetic for exciting irritation of the skin. He has constantly found it to produce the particular effect in view; and has never remarked any constitutional derangement or other inconvenience to result from its use. When employed as a stimulant lotion for ulcers, he usually covered them with charpie soaked in a solution of two or four grains to the ounce. For the formation of an issue he applied several layers of adhesive plaster, with a hole cut through them on the part, then filled the cavity with the salt slightly moistened, covered the whole with a fold of plaster and a proper bandage, and allowed the salt to act for six or eight hours. Irritation of the skin was produced by rubbing in an aqueous or spirituous solution, soon after which an eruption of papulæ or vesicles appeared, with an extensive scarlet efflorescence.

The muriate of zinc will probably be found a convenient and effectual escharotic; but Professor Hanke has exaggerated the dangers attending the employment of other escharotics, and in particular, has imputed to the nitrate of silver constitutional effects, which it certainly never does produce. If the professor is correct in his account of the eruption caused by rubbing the muriate of zinc into the skin, it will be a very convenient substitute for tartar emetic, as it does not appear to affect the skin so deeply.

The method recommended for preparing it is the following. Metallic zinc is to be dissolved in hydrochloric acid with the aid of heat, and the acid to be fully saturated towards the close by the addition of a little powdered carbonate of zinc. A solution of chlorine in water is next to be added, for the purpose of throwing down the iron with which the zinc is usually impregnated; and the fluid, after filtration, is to be evaporated by a gentle heat till it forms a firm gelatinous mass. This is to be preserved for use in close vessels, as it is deliquescent.

MEDICINE, MEDICAL PATHOLOGY, AND THERAPEUTICS.

Case of Hydatids of the Uterus. (Rust's Magazin für die gesammte Heilkunde.)—A woman, after being six times pregnant, imagined herself with child for the seventh time, in consequence of the cessation of the catamenia. Soon afterwards she was attacked with incessant vomiting, the abdomen increased rapidly in size, and became tender to the touch, the legs also became œdematous and the breathing short. These symptoms, however, she conceived to arise from twins, and she did not apply for medical assistance. In the fifth month of the supposed pregnancy the swelling of the belly became rapidly enormous, the œdema extended over the trunk, and her respiration became very difficult, when at length labour-pains suddenly set in, and considerable hemorrhage ensued, which was at first checked by cold applications, but soon afterwards returned more violently. At the same time a large mass of hydatids as big as two fists was discharged. The accoucheur, who now saw her for the first time found her pale, fainting, without pulsation at the wrist, and breathing slowly and laboriously. The fundus uteri was a hand's breadth above the navel, and the tumour formed by the womb was regular, not elongated or lying to one side as is usually the case in this disease. The os uteri was dilated to the size of a half dollar, and was very tender to the touch. Masses of hydatids projected from it into the vagina; and there was constant hemorrhage. The treatment adopted in these circumstances consisted in the administration of tincture of castor every fifteen minutes, spirituous frictions of the abdomen, and the injection into the uterus of a solution of salt acidulated with vinegar. In consequence the labour-pains, which had almost ceased, were speedily renewed and rapidly increased in strength, masses of hydatids were at the same time discharged, and ere long the hemorrhage abated considerably. In the course of two hours the fundus uteri had descended to within a hand's breadth above the pubis; in the course of the day it had descended still lower; the hemorrhage ceased; a warm perspiration broke out, accompanied with rising of the pulse, and the patient soon recovered. Milk appeared in the breasts on the third day, but receded again without any bad consequence. The hydatids weighed six pounds. They were attached to the remains of a *membrana decidua.*

On the Treatment of Amenorrhœa and Leucorrhœa. (Révue Med. Juillet 1827.*)*—The July number of this Journal contains two short but instructive papers on the subject of amenorrhœa and its concomitants, particularly leucorrhœa. The author of one of them, M. Moulin, considers chiefly the form of the disease which attacks robust, plethoric females, and which is characterized by the cessation of the discharge being speedily followed by a febrile state of the pulse, and symptoms indicating a determination of blood to one or other of the great cavities, if not actual inflammation of one of the viscera. In such cases it has long been the custom to use the lancet, and M. Moulin has given some excellent examples of the efficacy of such treatment. He states, that when blood was drawn on the day before that on which the flow of the catamenia, but for the suppression, would have begun, he has found it reinstated almost invariably, and sometimes the restoration took place while the blood was flowing from the arm. When leucorrhœa accompanied such cases, it ceased at once after the bleeding if the menstrual discharge was restored. M. Guibert, the author of the other paper, after a few observations, in which he concurs with M. Moulin as to the advantage and peculiar effects of bloodletting in the amenorrhœa of robust plethoric people, proceeds to notice the chief emmenagogues which he has found useful in the other variety, the amenorrhœa, namely, of females of a feeble, nervous, and lymphatic temperament. Of all the substances he has tried none have appeared to him to be so steadily successful as Venice turpentine. The form in which he uses it is that of pill compounded of two parts of turpentine, three of Castile soap, and the necessary quantity of liquorice-root powder, each pill containing four grains, and ten being the daily dose. Even in inveterate cases he has seldom been obliged to continue the treatment above twelve days.

Treatment of Mercurial Salivation. (North American Medical and Surgical Journal, January 1827.)—Dr Finlay of Bainbridge, in the province of Ohio, has proposed to employ the tartrate of antimony for checking mercurial salivation. He gives it every two hours in the dose of a tenth up to a sixth of a grain dissolved in water, so as to act gently on the skin and the alimentary canal; and the treatment is continued till the cure is completed. He reports that in this way he has in every instance immediately prevented the salivation gaining ground, has always procured marked relief in twenty-four hours, and in particular has in that time removed the pain of the mouth and throat. He has never found a single case resist the proposed method, and one of his cases, which had lasted three months, was cured in a few days. Others have formerly employed tartar emetic for the same purpose, but with little advantage. The success of Dr Finlay may have depended on the more gradual introduction of the remedy into the system, and the greater certainty of its action on the skin.

Sulphate of Quinine given in Large Doses. By Dr Perrine in Adams County, Natchez, Mississippi, (Philadelphia Journal, No. 7.) *and Dr George W. Stedman, Santa Cruz.*—Sulphate of quinine has been generally given in doses of from 1 to 2 or 3 grains. Chomel indeed gave it in doses of from 6 to 16 grains. But most practitioners have exhibited it in much smaller quantity. In our account of the reports of Professor Speranza we mentioned that it was given in large doses by the Italian physicians; that 12, 24, or 30 grains was not uncommonly given by those resident in aguish districts; and that by one practitioner it was carried to the unusual height of 108 grains at one dose before the fever was arrested. In similar situations in the New World similar liberality is observed. Dr Henry Perrine of Adams County, near Natchez, a district noted for the frequent prevalence of country sickness, has found it necessary to administer this remedy in doses of 6, 8, or 12 grains, repeated every two or three hours, while the symptoms in the pulse and skin are unrelieved. He almost invariably premises blood-letting to the amount of from 12 to 20 ounces, and clears the bowels with purgative pills or calomel. His medium dose of sulphate of quinine was 8 grains every hour. In one case 5 grains were administered every half hour till 20 grains were taken. In another instance 8 grains were administered every third hour till 64 grains were taken. The general effect of this remedy is to render the pulse fuller and less frequent, and to induce general and free sweating. Dr Perrine, after stating that his own pulse in ordinary health is 60 in the minute, informs us that when it was 80 during the exacerbation of fever, he tried on successive days the effect of an 8-grain dose of the sulphate in its frequency; and found the only variation, in two hours after each experiment, to be four beats less. Some time afterwards he was attacked by ague, at the beginning of the chill of which his pulse was 88, small and feeble. An 8-grain dose of sulphate of quinine was immediately taken. In fifteen minutes afterwards his pulse was 76, fuller and firmer; in one hour and fifteen minutes it was 64, and full; and in an hour more he felt well enough to ride out.

Scarcely had we collected these short notes, when we received from an intelligent correspondent, Dr George W. Stedman of Santa Cruz, information to the same effect. That island, it is well known, is, like the others of the West Indies, always subject to ague and remittent fever, and occasionally to the severe and epidemic form named yellow fever. "When I came out here," says Dr S., "I found the sulphate of quinine in great vogue; but it was seldom given in larger quantity than 6 or 7 grains in the course of the day. It hence often failed, and was even beginning to lose its reputation, when I boldly raised it to 10, 12, 14, and even 15 grains in the day with the most marked success. The most advantageous method of administering it is in the form of pills during the intervals. At bed-time a calomel purge is given, so as to operate by the aid of a little salts early in the morning. This obviates a distressing feeling of fulness in the abdomen, of which many patients complain after its use, and at the same time it prepares for a fresh load of quinine. It is extraordinary how different the sensibility of different stomachs is to this medicine. I have known some patients, particularly ladies, who were obliged to go to bed after six grains even, and who with difficulty could keep it on their stomach; while my practice furnishes an instance of a gentleman about twenty-seven years of age, who, mistaking my orders, took twelve grains at one dose, not only without any inconvenience, but to the total suppression of the fever."

Though we question neither the propriety nor the fact of the administration of these large doses of a powerful medicine; we think this is a good opportunity for saying, that in all cases in which the usual doses are ineffectual, or larger ones become requisite, it is desirable for the practitioner to be well assured that his medicine is genuine and of good quality.

SURGERY AND SURGICAL PATHOLOGY.

Case of Protracted Epilepsy cured by removal of a depressed portion of bone.—(*New-York Medical and Physical Journal*, 1826.—*1st Quarter.*)—Dr Rogers of New-York has given the following interesting account of a case in which epilepsy of very old standing and very violent was removed entirely and rapidly by elevating a portion of the *os frontis*, which had been depressed upon the brain. The subject of it was a gentleman, forty-six years of age, who fourteen years before had sustained a depressed fracture of the skull in consequence of a blow with an iron bar. As there were not any symptoms of compression of the brain the depressed bone was not interfered with, and in a few weeks he recovered. Ere long, however, he was attacked with epilepsy, which for the first two or three years occurred only once a month, and was attended with good health and clearness of intellect in the intervals, but which gradually increased in frequency, severity, and duration, till at length he had two or three daily, and lost almost entirely his memory. He likewise complained constantly of pain extending down the neck and arm of the affected side, and likewise of pain in the seat of the fracture, increased on the approach of the fits. The seat of the fracture was immediately over the left *supra-orbitary foramen;* and through the integuments a depression could be felt which admitted the point of the finger. An operation being resolved on, a circular piece of bone was removed involving the upper part of the frontal sinus, and having attached to it a lamina of bone, which had been fractured at its lower connexion with the posterior plate of the frontal sinus, and beat inwards, so as to press on the brain. In consequence of the great inequality of the thickness of the circular piece of bone, a pretty extensive incision of the dura mater was made with the instrument. The membranes around were unusually vascular.—The antiphlogistic regimen being then carefully observed, the patient continued to improve steadily without any alarming symptom, and *without any return of his epileptic fits* till the twenty-fifth day after the operation, when, in consequence apparently of having overloaded his stomach, he had one fit attended with high arterial excitement and followed by inflammation of the brain. Vigorous depletion, however, reinstated him, and he was considered to be cured seven weeks after the operation was performed. Nine months afterwards the cure continued perfect, his memory was almost entirely restored, and altogether he was so well as to be able to attend to his business without any inconvenience.

It is seldom that the seat of the local mischief, which causes epilepsy in those cases which arise from organic derangements within the head, can be exactly ascertained; and it is not always that, when ascertained, they are within the reach of a surgical operation. But the foregoing case, added to others of a like kind on record, shows remarkably how much may be affected when the source of the disease can be reached, and how little we need be deterred by the fear that organic changes may have been produced in the brain itself by the long continuance of mechanical irritation.

Spontaneous Emphysema. (Rust's Magazin für die gesammte Heilkunde, 1826.)—A stout man, 59 years old, was attacked with cough, accompanied with acute pain in the right side, and a sensation there as if something was forced outwards at each cough. He did not apply for medical advice, however, till he remarked a tumour in the seat of the pain. When he was first visited by his physician, the pain was felt on a small spot between the seventh and eighth ribs of the right side, a few inches from the spine; the whole breast and side of the chest were much swollen, the swelling being evidently emphysematous, and when the tumour was pressed down with the finger, over the seat of the pain, it was at once restored with the next fit of coughing. The emphysema gradually extended day after day, till the whole trunk and head, both arms, and even the right thigh were affected. As suffocation seemed then to threaten the patient's life, incisions were made in various parts, and air escaped from the apertures when the body was moved, or when pressure was made around them. On the back, where the swelling was greatest, incisions could be made an inch and a half in depth without coming in contact with the muscles. The difficulty of breathing was thus removed; but the disease continued some weeks longer, till at length an incision was made near the spot where it was presumed the air escaped from the chest. Inflammation of the part was thus induced, terminating in induration of the cellular tissue and closure of the opening.

Case of Ligature of the Carotid for supposed aneurism, proving fatal by laceration of the artery under the ligature. (Nouvelle Bibliotheque Médicale, Mar. 1827.)—The case we are now to describe occurred in the hospital practice of M. Lisfranc. A young girl had a tumour of the size of a pigeon's egg behind the right angle of the lower jaw; and as it presented to M. Lisfranc all the characters of an aneurism he resolved to tie the common carotid This was accordingly done, without any untoward accident happening; the ligature consisted of a single thread; and the wound was dressed so as to heal by the first intention. A few hours after the operation symptoms of inflammation of the brain arose, attended likewise with inflammatory symptoms referred to the throat and chest; but these were all subdued by the antiphlogistic treatment, and the girl on the eighth day appeared to be doing well, the tumour having diminished considerably, when, on bending the head to drink, she felt a sense of something giving way in the wound, and a profuse hemorrhage ensued which proved speedily fatal. On dissection it was found that the tumour was not an aneurism, but a fungus hæmatodes, of which a considerable portion occupied the middle cerebral fossa of the right side, the petrous portion of the temporal bone being mostly destroyed or converted into fungoid matter. The internal jugular vein was filled for six inches of its course with a polypus, adhering by two or three pedicles. In the carotid artery between the ligature and the heart there was a lacerated hole three lines long, partly closed by a fibrinous clot. The ventricles of the heart were much enlarged, and their parietes thinner than natural. There was in the posterior mediastinum an abscess containing a wine glassful of pus.

Employment of Artificial Respiration in the Treatment of Poisoning with Opium. (North American Medical and Surgical Journal for July 1826.)—Mr Brodie succeeded in restoring to life and health by means of artificial breathing two animals which had been brought so completely under the influence of narcotic poison, that without some such treatment they must have perished. Encouraged by this result, Dr Ware of Boston [U. S.] resorted to this measure in the case of a child affected with chincough, who had taken an over-dose of opium. The pulse had ceased at the wrist; the action of the heart was at the time reduced to a few pulsations with long intervals between them; and the respirations were also exceedingly few and imperfect. Artificial respiration was then tried and maintained for a few minutes, upon which the pulse returned at the wrist, and the pulsations of the heart became manifest. In the course of an hour, the breathing having been resumed at intervals, the respiration became natural, the pulse distinct and pretty strong, and the heat also natural. In the midst of this amelioration, the little patient was attacked with a violent fit of hooping-cough, which ended in suspension of the breathing. The insufflation of air was again resorted to, and with the same good effects as before. The fits, however, continued to recur with great violence, and at length the child was carried off in the middle of one. There is little doubt but the means employed against the narcotic effects of the opium would have been attended with the most flattering result, had they not been rendered unavailing by the accompanying disease.

Urinary Concretion of unusual dimensions.—We have been favoured by Dr John King of Irvine with the inspection of a urinary calculus of a very extraordinary size. Its weight after being thoroughly exsiccated was 13,000 grains, or 27 ounces 40 grains. Its length was 5.56 inches in one direction, its diameter was 3.37 inches, and in another direction 3 inches. Its longer circumference was 15 inches, in another direction 14.12 inches, and its transverse circumference 10.37 inches. It was externally of a grayish white colour, and its surface slightly tuberculated.

The patient from whom this stone was taken after death was from his infancy afflicted with severe pain in voiding his urine. The pain did not remarkably increase until about the fourteenth year of his age, when the symptoms becoming more severe, he had recourse to the usual palliatives, but found no permanent relief.

He continued his usual occupation (which was that of a carter) for the space of twenty-six years afterwards, never exempt from the most acute pain. In the earlier part of this period he was repeatedly advised by several medical gentlemen and Dr King to undergo an operation, but to which neither he nor his relations would consent.

During the last six years of his life he was unable to follow his ordinary occupation; and was confined to bed, and when, about four months before his death, Dr King was again called in, he told him he was willing to submit to any thing that would tend to give him ease. But from the enlargement of the stone, which could be felt externally Dr King deemed an operation impracticable. On his death Dr King requested leave of his relatives to inspect the body, which was readily granted (as it had been his dying request.) He found the inguinal glands diseased, and, on laying open the bladder, the stone above described. He died in the forty-sixth year of his age.

Lithotrity and Stone-drilling—As the exact date and the precise circumstances of the case of Colonel Martin appear to be viewed with doubt by the authors of the Report on the work of M. Civiale, we give the following account, communicated *verbatim* by Dr Monro from his father's manuscript lectures, in which we have seen the case. This reference proves indisputably that the date of Colonel Martin's operation was not later, whatever sooner, than 1800.

"Before I conclude this subject, it may be worth while to mention the case of an officer of high rank in India, (Colonel Martin) who persuaded himself that he might be able to rid himself of a stone in his bladder by rasping and cutting it with a file. He accordingly procured a steel sound of the ordinary length and shape, and about one-tenth part of an inch in its diameter, which was made rough like a file on its convex part and sides for the length of three quarters of an inch from its point. He with much courage and perseverance introduced this into the bladder four or five times a-day, filing the stone for half an hour or so each time, and persevered in doing so for nine months. During that time he rasped off a great deal of powder, and cut off a number of small pieces from the stone, several of which he sent me in a letter under cover to the Right Honourable Sir J. Sinclair, along with the instrument he had employed. He was unfortunately seized at the time he wrote me with liver complaint, of which he died soon after without having destroyed or discharged the whole stone."

"It appears that the stone had been generally lodged within and grasped by the sphincter of the bladder, in which situation he could reach it and generally file it without its slipping backwards into the cavity of the bladder."*

"Notwithstanding the progress the Colonel had made in his case, I apprehend there are few readers who would think his method should be prosecuted, and stil fewer who would have the courage to practise it."

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* "Colonel Martin had corresponded with Sir J. Sinclair and sent him a copy of the letter I received, dated Lucknow, Setember 5, 1800."

As far as Dr Monro can recollect these filings were uric acid; consequently the stone was probably hard.

The following Letter from Mr Liston contains an account of the first case in which the operation of Stone Drilling has been performed in this country.

DEAR SIR —I send you as I promised an account of the case in which I lately performed *Lithotritie.* The patient Peter Runciman, æt. 62, applied to me in the end of July last with symptoms of stone, which he had experienced, as he said, for only four or five months. At that time he was sounded, and the operation of lithotomy proposed, but he was very averse to submit to it. I had a few days before returned from Paris, where I witnessed the operations of Dr Civiale, and had ordered a set of his apparatus for grinding down the stone in the bladder. I had previously obtained the instruments of Lukens, an American, for the same purpose; but until I saw those of Civiale and their application, I confess that I had no very good opinion of the operation.

Runciman's case, from the short duration of the symptoms, appeared to be a very favourable one for the grinding process, and he very willingly agreed to submit, as soon as the apparatus arrived. The duration of the symptoms I say encouraged me to hope that the stone was of no large size; for in the adult it is no easy matter, whatever may be said by system writers, to ascertain by the rectum or otherwise the exact volume of a calculus. Neither is the duration of the painful feelings to be much relied on, as the present case will shew. I may also mention, that I have in my collection stones of very small size extracted from patients above twenty, who had laboured under all the symptoms from infancy; and this morning I cut a young gentleman, who has laboured under the disease since ever he can recollect, and removed a calculus not larger than a walnut. The apparatus of Civiale is graduated on the stalk, and is by no means a bad *Lithometer*, if I may use the term. But even with it one may be very much deceived as to the size of flat or irregularly formed stones.

Having accustomed the urethra to the presence of the straight sound by two or three introductions of it at intervals of some days, I performed the first operation on the 26th of September. The bladder being previously injected, I passed the instrument without difficulty; and in fact a little practice will enable any surgeon to introduce a perfectly straight instrument into the bladder, with equal facility and as little pain to the patient as a curved one. The stone was at once seized, drilled, slipped, caught, and drilled again, without the patient complaining of any but slight pain. I was not more delighted with the success than the patient, who immediately after walked home, a distance of two miles. The irritation which followed was not great, and was soon subdued by the use of diluents, and camphor and opiate suppositories.

On the 9th October the operation was repeated, and again on the 18th, 27th, and 31st; as also on November 6th, 13th, and 17th, with various success. At these operations, I was honoured by the presence of many of the most eminent medical men of this city. At each sitting the stone was seized and drilled, and generally without difficulty or pain. From the expansion of the instrument in the operation on the 26th of September, I supposed that the stone was about the size of a walnut. On the last, and one or two other trials, I imagined that it was reduced in size, as I had hold of it pretty firmly with the prongs of the litholabe nearly closed. The shape and indented state of the stone accounts satisfactorily for the deception.

On the 22d November, I sounded the patient very carefully, and was satisfied from the rub over the surface, that there was still a large calculus in the bladder. The old man was now impatient, and talked of being cut. To this I was by no means averse, and accordingly performed lithotomy on the 25th. The operation rendered more tedious from the impressions of the lithotritor on the edges of the stone, was finished satisfactorily in three minutes. The recovery of the patient has been very rapid, which I attribute in a great measure to the use of the tube which I recommended strongly in a former number of your Journal as preventing infiltration of urine, and which I have since then used in above twenty cases with complete success. I may be permitted to remark, that out of all the patients, a considerable number on whom I have performed the operation of lithotomy, both before and since the publication of the paper I allude to, I have never heard one complain of uneasiness in the hypogastric region, and have never but in one instance, where it was more a precautionary measure than otherwise, had occasion to bleed either generally or locally.

I regret much that the period of your publishing will not admit of my furnishing you with a drawing of the calculus removed from Runciman ; for no description can convey a correct idea of its size and appearance. But if you wish it I shall provide one for the next number. It is of a flat spheroidal form, much larger than I could have supposed, and composed almost entirely of uric acid. The measurements are in length, two inches and a quarter, breadth, one inch and three quarters, the thickness eight-tenths. The marks of twelve drillings are distinctly visible on different parts of the calculus, many of them very deep. One has passed entirely through, and four others all but through the smallest diameter.

I find on trials with a stone of the same size and shape that no secure hold can be taken of it but with the flat side towards the drill. In any other position it saps from the chops of the instrument immediately on the drill being set in motion. The result of this case rather confirms than otherwise the favourable impression I lately entertained of lithotrity. Had the calculus in this case, though of an unfavourable shape, been less hard and tenacious it must have been destroyed by the fourth or fifth operation. I have no doubt but that *Lithotritie* in many instances will naturally supersede *Lithotomy*, the dangers of which Dr Civiale in his work very much exaggerates. I shall certainly make further trials of the former operation and communicate the results to you.—I am, &c.

Successful Amputation at the Hip-joint. By Dr Valentine Mott, New York, (The Philadelphia Journal, No. ix. Vol. v. New Series.)—This is a case of amputation which was performed first through the small trochanter, and immediately after, without removing the patient from the table, through the articulation. The subject of it was a boy of 10, who had been previously healthy, but had suffered fracture of the right thigh-bone about two-thirds of its length from the hip joint. Two days after splints and bandages were firmly applied ; but they produced much pain, and at the urgent request of the patient they were removed. It was then proposed to apply the splint of Desault modified by Dr Physick. At this time it was remarked, that the fracture had been so badly reduced, that the extremity of the upper fragment was forming a swelling on the outside of the thigh, and was nearly forced through the integuments. When this mal-position was rectified as much as possible, the long splint was applied.

About three weeks after this the patient was seen by another physician, who recommended the use of the inclined plane, (double we imagine.) After being confined to this for several weeks, in the course of moving rather incautiously, the thigh is stated to have been injured immediately above the internal condyle, and an opening leading sinuously to the fractured bone took place. It is justly observed by the reporter, that this sinuous abscess had been probably formed, and ready to open when the slight injury completed it.

He was now brought to New York, where he was seen on the 7th September 1824 by Dr V. Mott. At this time the right limb was much enlarged on the outside, resembling the swelling of *spina ventosa ;* the surface was hard and irregular, but exceedingly tender, and the seat of excruciating pain when pressed. The swelling extended to the great trochanter, gradually diminishing towards the top of the thigh. Opposite the greatest enlargement was a sinuous opening discharging a thin sanious fluid, and leading to the middle of the thigh-bone, which was perfectly carious. The countenance was expressive of much anguish ; the tongue was white ; the pulse feeble ; and night-sweats, and the other symptoms of hectic were present. During two weeks after his arrival in New York, medicines were administered with the view of allaying irritation, and imparting some tone. But the hectic symptoms became worse ; and ulceration began to occur by the side of the tibia. It was then determined to remove the diseased limb at the hip-joint as the only chance of saving life.

On the 7th October 1824, the patient, after passing a comfortable night, underwent the operation in the following manner :—An incision was made over the femoral artery as it emerges from the arch, and the vessel was tied. While feeling on the outside of the artery for the small trochanter, the pulsation of a vessel, apparently little smaller than the femoral artery, immediately below the ligature, convinced the operator that in this case the *profunda* was given off above the femoral arch. This vessel was accordingly secured. Lisfranc's knife was then introduced between the artery and the bone, and carried through close to the neck of the thigh bone, towards the tuberosity of the ischium, so as to form the inner flap. At this stage of the process some of the surgical attendants examining the small trochanter pronounced the head of the bone to be *not diseased*. To obviate this doubt the bone was sawed through the small trochanter, when it was found to be of the consistence of cheese, and denuded of periosteum on the outside up towards the joint. When the outer flap, therefore, was completed, which was done by cutting from without inwards, the head of the bone was detached from the acetabulum in the usual manner, as originally contemplated. The hemorrhage from the veins and small arteries was considerable when the incisions were made, and numerous vessels were tied; (they appear to have been fifteen.) But little blood was lost during the operation, and the patient was put to bed shortly after it was completed.

The subsequent treatment consisted in regulating the diet, and administering anodyne and tonic medicines, according to circumstances.

On the 15th of October, eight days from the performance of the operation, two-thirds of the stump was healed by the first intention. Between the 17th and 31st October all the ligatures, seventeen in number, were removed; and by the 20th November the whole wound was effectually healed; and the boy had recovered flesh and become fat. Dr Mott adds, that there can be no doubt that this limb might have been saved had proper treatment been employed at the time of the accident. When it came under his charge nothing short of the operation could have saved the life of the boy.

This must be regarded as the first case of amputation of the hip-joint in which the patient completely recovered after the operation. In the case of Sir A. Cooper, which was also successful, there was this difference, that the limb had been previously amputated near the trochanters.

Fatal Hemorrhage from the rupture of a Varicose Vein. [*Gazette de Santé*, 5 *Mai* 1827.]—A woman fifty-four years of age, of a lymphatico-sanguine constitution, robust habit, and usually enjoying excellent health, had been liable for many years to varix of the veins of the legs; and, in particular, she had on the middle and inner part of the right leg a varicose tumour as big as a hen's egg, and on the corresponding part of the left leg another as large as two thumbs. These varicose swellings increased very much after the catamenia had ceased to flow, probably in consequence of her having for some time abandoned the custom of getting herself bled periodically. At last the tumour on the right leg was accidentally lacerated, and she lost a pint of black fluid blood before the hemorrhage could be stopped by a compress. An ulcer succeeded the wound, from which a sero-sanguinolent fluid issued to the amount of two cupfuls every day. The tumour however gradually decreased, and as it shrunk, an erysipelatous inflammation commenced in the part, and spread over the whole limb. This disorder, too, was in the end successfully treated, and at length the limb was restored to its natural condition without a vestige of the varicose tumour remaining. But the varix of the left leg continued undiminished. One day, in consequence of the part itching much, she was led to scratch it, upon which blood suddenly sprung from it with force, and continued to flow continuously and rapidly, notwithstanding the employment of such means as were at hand. In a short time she lost seven pints of blood, and then she fainted. On recovering from her faint, the hemorrhage returned till she fainted again; and the ignorant people about her having contented themselves with simply wrapping the limb in cloths, without compressing it, the blood continued to ooze through the dressings, and the woman to fall into one faint after another, till at length the pulse failed, the skin became cold and blanched, the sight confused, the breathing feeble and interrupted, and shortly afterwards she expired.

No. XCV. will be published on the 1st of April.

THE

EDINBURGH
MEDICAL AND SURGICAL JOURNAL,

No. XCV.

CONTENTS.

PART I.—Original Communications.

PAGE

Art. I. Observations on the Operation of Lithotomy. By John Crichton, Esq. Surgeon, Senior Surgeon to the Royal Infirmary of Dundee, 225

II. List of Cases in which the Operation of Lithotomy was performed. By Robert Liston, Esq. Lecturer on Surgery, one of the Surgeons of the Royal Infirmary of Edinburgh, &c. &c. &c. 236

III. Observations on the best Method of accomplishing Delivery in Presentations of the Superior Extremities, where turning is unadvisable or impracticable. By Robert Lee, M. D., - 239

IV. Observations relative to the questions, Is the Fibrin of the Blood of lower specific gravity than even the serum ? Is the Blood which has a Buffy Coat after coagulation of lower specific gravity than Healthy Blood ; and does it contain a larger proportion of Fibrin ? By John Davy, M. D., F. R. S., Physician to the Army, 244

V. Observations relative to the question, "Is there any heat given off during the coagulation of the blood ?" By John Davy, M. D., 250

VI. Observations relative to the Question, Is there any free Carbonic Acid in the Blood ?" By John Davy, M. D., - 253

VII. Observations, Pathological and Practical, on Whitloe. By David Craigie, M. D. - - - - - - 255

VIII. Case of Diffuse Inflammation of the Cellular Membrane from a Poisoned Wound, terminating favourably under the use of deep incisions. By John Evans, M. D. - - 281

IX. Case of Amputation of the Thigh during the progress of Gangrenous Inflammation. By George Macdermott, Esq. Surgeon, 289

X. Case of Obstruction in the Arteries from an internal Cause. By James Syme, Esq. - - - - - 291

XI. Case of Tubercular Tumour of the Brain terminating in Hydrocephalic Effusion. By Robert Evans, M. D. - - 295

XII. Case of Hypertrophy of the Heart, with Cartilaginous Induration of the Mitral Valve. By Henry Vernon, M. D. M. R. C. S. L. 300

XIII. Case of Active Hypertrophy of the Heart. By James Black, M. D. of the Royal College of Physicians, London, - 303

XIV. Case of Organic Disease, chiefly in the Ovaries. By Alexander Murray, A. M. - - - 305

XV. Case of Fungus Hæmatodes in the Kidneys. By Ebenezer Gairdner, M. D. - - - 312

XVI. Remarks on Strumous Corneitis and Iritis, with Cases. By James Guthrie, Esq. Surgeon, - - 315

XVII. Case of Laborious Parturition, complicated with Laceration of the Uterus and Vagina. By William Campbell, M. D. - 320

XVIII. On the Efficacy of Spurred Rye in inducing Uterine Action. By Robert Renton, M. D. - - - 322

XIX. Suggestions for the Improvement of the Female Catheter. By William F. Montgomery, A. M., M. B., - - 325

XX. Notice of the Appearances in the Brain of a Young Female who attained her seventeenth year without giving Indications of Sensation or Motion from Birth. Communicated by Davies Gilbert, Esq. M. P. - - - - - - - 438

XXI. Case of Pertussis, with the Dissection of the Body, in which was found, besides the Morbid Appearances, a Remarkable Transposition of the Thoracic and Abdominal Viscera. By William Moncreiff, M. D., &c. - - 440

PAGE

PART II.—CRITICAL ANALYSIS.

ART. I. Pathological and Practical Researches on the Diseases of the Brain and the Spinal Chord. By JOHN ABERCROMBIE, M. D. 329
II. Ueber die Punction des Chronischen inneren Wasserkopfs, &c. On the Operation of Puncture in Chronic Hydrocephalus. By Dr FREDERICK WILLIAM OPPENHEIM, Physician and Surgeon, 358
III. Illustrations of the Anatomy of the Male Pelvis. By ALEXANDER MONRO, M. D., F. R. S. E., - - 366
IV. Observations on the Mortality and Physical Management of Children. By JOHN ROBERTON, Member of the Royal College of Surgeons, Edinburgh, - - - - 373
V. 1. Traité Theorique et Pratique des Maladies de la Peau, &c.—Treatise, Theoretical and Practical, on Diseases of the Skin; founded on new anatomical and pathological researches. By P. RAYER, Titular Physician of the Dispensaries of the Philanthropic Society.
2. A Practical Treatise on the Diseases of the Skin, arranged with a view to their constitutional causes and local character, &c. &c. By SAMUEL PLUMBE, M. R. C. S., &c. - 385
VI. A Practical Essay on Stricture of the Rectum, illustrated by cases, showing the connection of that disease with affections of the urinary organs and the uterus, with piles, and various constitutional complaints. By FREDERICK SALMON, Surgeon, - 399
VII. A Treatise on the Diseases of the Chest and on Mediate Auscultation. By R. T. H. LAENNEC, M. D. &c. Translated from the French, with notes, and a sketch of the Author's Life. By JOHN FORBES, M. D. - - - - 406
VIII. Reports of the Proceedings in the High Court of Justiciary. By DAVID SYME, Esq. Advocate, - - 410
IX. Researches into the Causes, Nature, and Treatment of the more Prevalent Diseases of India, and of warm Climates generally. By JAMES ANNESLEY, Esq. M. R. C. S. and M. R. M. S. - 419
X. A Corn. Celsi Medecinæ libri octo ex recensione Leonardi Targæ. Quibus accedunt, &c. &c. Concinnavit EDVARDUS MILLIGAN, M. D. S. A. S. S. &c. &c. &c. - - 436

PART III.—MEDICAL INTELLIGENCE.

On Cutaneous Absorption, - - - 444
Nature and Composition of the Sweat and Insensible Perspiration, - 445
On the effects of extirpating the Kidneys, - - 446
Analysis of the Mineral Waters of Scotland, by Dr THOMAS THOMSON, ib.
On the period of the disease at which Measles begins to be infectious, - 447
On the Treatment of Intermittent Fever by the application of Sulphate of Kina to the Skin, - - - - 448
On the use of Tannin as a remedy for Menorrhagia, - - 449
Case of Death caused by the external application of Opium, - ib.
Treatment of Poisoned Wounds, - - 451
Transfusion of Blood, - - - - ib.
Cases of Poisoning with Belladonna, - - - 452
On the effects of injections of the Eustachian Tube in the treatment for the removal of Congenital Deafness, - - ib.
Extra-uterine pregnancy, terminating in the discharge of the fœtus piece-meal by the anus, - . - - 453
Case of Double Uterus, with Double Impregnation, - 454
On the Relation which the Mean Stature and general perfection of the body bear to rank and mode of life, - - - ib.
Table of the diseases which cause death at various periods of infancy, - 455

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr Rodman, Dr Winterbottom. Mr Dewar, Mr Alexander, Mr Leah, and Mr Burt.

The following publications have been received:—

Traité generale d'Anatomie Comparée par J. F. Meckel. Traduit de l'Allemand, et augmenté de notes, par MM. Hiester et Alph. Sanson Docteur de Chirurgie de la Faculté de Paris. Precedé d'une Lettre de l'auteur, Tome I. Paris, 1828.

Transactions of the Medical and Physical Society of Calcutta, Volume III. Calcutta, 1827.

A Manual of Surgical Anatomy, containing a minute description of the parts concerned in Operative Surgery, with the anatomical effects of accidents and instructions for the performance of Operations. By W. M. Edwards, D. M. P. Translated with notes, by William Coulson, Demonstrator of Anatomy at the Medical School, Aldersgate Street. London, 1828.

A Compendium of the diseases of the Human Eye, containing descriptions and explanations of the various diseases; illustrated by engravings, and accompanied by practical observations on their treatment. By Alexander Watson, Fellow of the Royal College of Surgeons, Edinburgh, &c. &c. &c. 2d edit. much enlarged. Edinburgh, 1828.

Observations on Cow-pox, and the necessity of adopting legislative measures for enforcing Vaccination. In a letter to Mr Thomas Brown, Surgeon, Musselburgh, containing remarks on his "Letter to the Right Hon. the Earl of Liverpool, concerning the present state of Vaccination." By Henry Edmonston, A. M., Surgeon, Newcastle-upon-Tyne. London, 1828. 8vo.

Practical Observations on Insanity, and on the treatment of the Insane; addressed particularly to those who have relatives or friends affected with mental derangement. Also, Hints on the propriety of making the study of mental diseases a necessary adjunct to medical education. By W. J. late keeper at a Lunatic Asylum. London, 1828. 8vo.

A Manual of the Physiology of Man; or a concise description of the phenomena of his organization. Translated from the French of Ph. Hutin. London, 1828.

Observations on the properties and effects of the expressed oil of the seed of Croton Tiglium; together with the botanical history, and a correct coloured engraving of the plant. By John Frost, F. S. A. F. L. S. F. H. S. of Emmanuel College, Cambridge, &c. &c. &c. London, 1827.

Journal de Progres des Sciences, et des Institutions Medicales. Volumes V. and VI.

The North American Medical and Surgical Journal. Conducted by Hugh L. Hodge, M. D.; Franklin Bache, M. D.; Charles D. Meigs, M. D.; B. H. Coates, M. D.; and R. La Roche, M. D. No. 8. October, 1827. Philadelphia, 1827.

NOTICES TO CORRESPONDENTS.

The American Journal of the Medical Sciences, No. I. November 1827. Philadelphia, Carey, Lea, and Carey; London, John Miller, 40, Pall Mall; Paris, J. B. Bailliere, Rue de l'Ecole de Medicine. 1827.

The Philadelphia Monthly Journal of Medicine and Surgery for November 1827. Edited by N. R. Smith, M. D. Professor of Surgery in the University of Maryland. Philadelphia; R. H. Small, 165, Chestnut Street.

A Clinical Lecture delivered to the Students of Surgery in the Royal Infirmary of Edinburgh, at the conclusion of the Winter Course for 1827-1828. Edinburgh, March 1827. By George Ballingall, M. D. F. R. S. E., &c. Regius Professor of Military Surgery in the University of Edinburgh, &c.

Abbildungen der Ruckenmarksnerven nebst Erklärung der Sieben Kupfertafeln. Von D. August Carl Bock Prosector am Anatomischen Theater der Universitat zu Leipzig. Leipzig in Baumgarteners Buchhandlung, 1827. Gedrucht bei J. B. Hirsfield. Large Folio. These Engravings of the Origin, Course, and Distribution of the Spinal Nerves, which are neatly and distinctly represented, we shall probably notice more particularly on a future occasion.

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PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*Observations on the Operation of Lithotomy.* By John Crichton, Esq., Surgeon, Senior Surgeon to the Royal Infirmary of Dundee.

THE intensity of the sufferings occasioned by urinary concretions in the bladder, and the inefficiency of medicine to afford relief, have, from the earliest ages, led to the employment of the knife for their removal.

Till towards the end, however, of the 17th and beginning of the 18th century, the methods practised, in consequence of the imperfect state of anatomical knowledge, were rude, and attended with much danger. But about that period we find surgeons in this country and in France zealous in their profession, and animated by the interest taken by princes and parliaments in their proceedings, rivalling one another in their exertions to ascertain, by means of numerous dissections and experiments, in what manner this removal might be accomplished with the least possible pain and danger to the unfortunate sufferers. The result of their labours was what has been called the method of Cheselden, or the plan of gaining access to the bladder from the space between the left *ramus ischii* and the rectum, by the division of the membranous portion of the urethra and of the prostate gland.

However simple in the mode, rapid in the execution, and successful in the result, this method of operating is understood to have been in the hands of Cheselden, succeeding operators do not appear always to have experienced the same facility in the execution and success in the result; if we may judge from the numerous improvements, modifications, and alterations in the form of instruments that have been at different times proposed by different operators, and from what we hear of the extraordinary length of time often occupied in the performance. Yet we learn from Dr Douglas that Cheselden seldom occupied more than one minute from the first incision to the extraction of the stone; and that this is practicable there can be no doubt, having myself frequently finished the operation within that space of time.

To what, then, are we to attribute these increasing difficulties? difficulties so great in the estimation of some, as to induce them to abandon the method of Cheselden altogether, and to have recourse to the old exploded practice of distending the urethra and neck of the bladder, instead of dividing the parts, of seeking access to the bladder through the coats of the rectum, by cutting into it above the pubes, or latterly by introducing instruments into the bladder for the purpose of breaking down the stone? Much no doubt depends upon the surgeon himself. In an operation such as this, unaided by the sight, more abstraction of mind and intensity of thought are necessary for its right performance, than the generality of surgeons engaged in the routine of practice are able or willing to bestow There is reason likewise to think, that the very multitude of proposed improvements, and the directions given in many of the writings upon the subject by authors of high repute, have not a little tended to perplex the minds of surgeons, and to create erroneous impressions on the nature and effects of the operation.

For instance, John Bell, in his elaborate volume upon lithotomy, after very properly enforcing the propriety of studying the anatomy of the parts by frequent dissections upon the dead subject, before attempting to operate upon the living, delivers the following opinion, "No operation in surgery can so properly be defined a *dissection* performed upon the living body as this;" and again, p. 197, "If it were his design to operate only with the knife, the surgeon would now push his finger deep into the wound, and, by the help of the fore-finger, *dissect* from the urethra along the body of the prostate gland."

Now the idea which we attach to the dissection of a part is its slow and cautious division, fibre by fibre, for the purpose of exhibition and gaining information. The dead subject feels no pain; and no inconvenience can result from tedious and protracted incisions. But in operating upon the living body, the surgeon is supposed already in possession of the requisite information; and, therefore, all unnecessary delay, and every unnecessary incision, must be considered unwarrantable cruelty, as well as adding to the danger of the operation. Two, or at most three, strokes of the scalpel are sufficient to lay bare the membranous part of the urethra; and certainly the less the parts are mangled, and the more direct and continuous the opening is, the less risk there will be of urinary infiltrations and sloughing sores.

Much too has been written and said relative to the manner of dividing the prostate, and the form of instrument best adapted for the purpose. The alterations that have from time to time taken place, and the claims of superiority for one mode or form of instrument over that of another, by the different inventors and their respective abettors, prove sufficiently amusing, were it not in consideration of the mischief they occasion by withdrawing attention from what is chiefly of importance, namely, intimate knowledge of the parts, and self-possession during the operation.

All the different modes of dividing the prostate gland that have been proposed may be comprehended under the following heads. 1st, Making the incision from its outer surface towards the cavity of the bladder, as is done by the common scalpel and the different modifications of the cutting gorget. 2d, Cutting from within the bladder outwards, as is effected by the *bistoire cachée*; and 3d, Cutting the prostate from the surface of the portion of urethra, contained within the gland, by using the narrow probe-pointed knife. By each of these modes the opening has been successfully made, and will be so by those who can properly guide the instrument, and by none of them will it be well executed by those who cannot.

The directions given by different authors, in regard to the mode of finding and laying hold of the stone, demonstrate the confused and erroneous notions entertained on this point.

According to Heister, "In the lateral method and apparatus major, the surgeon is often a long time searching with the forceps for the stone in the dark."

Benjamin Bell, in his System of Surgery, Vol. ii. page 105, says, "As soon as the forceps have entered the bladder, they should be gradually opened; and in this expanded state ought to be easily moved about with their handles, sometimes depressed, sometimes elevated, till the stone is discovered."

Samuel Cooper, in his Practice of Surgery, page 483, says, "The next object is to take hold of the stone with the blades of the forceps. In order to accomplish this purpose, the operator should not expand the instrument as soon as it has entered the bladder; but he should make use of the forceps as a kind of probe for first ascertaining the exact situation of the stone, &c. &c. No man can doubt that the injury which the bladder frequently suffers from reiterated and awkward movements of the forceps is not an uncommon cause of such inflammation of this viscus, as is too apt to extend to the peritonæum and occasion death," &c. &c.

J. C. Carpue, in his History of the High Operation, page 139, (speaking of the lateral method,) says, " This part of the operation is frequently tedious, and often attended with the most serious consequences, dreadful to the patient, and most distressing to the operator, as the stone may be situated in the posterior of the bladder between the *vasa deferentia;* it sometimes lodges above the prostate gland behind the pubes. In a case which occurred in my presence the stone could not be found."

From these, and many other extracts from writings upon the subject that might be adduced, there appears a great want of clearness of conception upon this point. Directions are given as if the stone were to be searched for in a distended bladder, or in an empty space. Whereas the real state of the matter is simply this: When the bladder is laid open the urine escapes, and the bladder contracts around the stone. Where, then, is the stone to be found? Close to the opening most certainly. In nearly one hundred cases wherein I have operated it has always been so. How comes it then we so often hear of difficulty and delay in finding it? By not adverting to this plain circumstance, the forceps are at one time passed over the stone carrying the coats of the collapsed bladder before their point; they are opened and shut, shut and opened; they are pushed this way and that way, and still nothing is found; the stone all the while lying behind the blades. At another time the stone is pushed before the points of the forceps, and a portion of the bladder contracting around it like the finger of a glove, it cannot be laid hold of. The like may also possibly happen from the bladder being too much distended with urine, in consequence of the cruel and absurd practice recommended and sometimes adopted, of compressing the urethra for some hours before operating, in which case the bladder, when suddenly emptied, may collapse irregularly, and retain the stone, especially if it be a small one, betwixt its folds.

The risk of wounding the pudic artery, and thereby occasioning fatal hemorrhagy, which has of late been much dwelt upon as an argument against this method of operating, does not appear to me of great force, and, I should think, ought to be of very rare occurrence.

In the course of thirty-six years I have not experienced any such accident, or indeed hemorrhage of any sort, which was not always readily commanded, either by securing the divided vessel by ligature, or by the application of cloths soaked in cold water to the scrotum.

Amid the numerous writings upon this subject, there is one circumstance, and that of no inconsiderable importance, which does not appear to have been much agitated,—namely, the propriety of accomplishing a cure by the first intention.

It is well known that a great portion of the distress attendant upon the operation arises from the dribbling of the urine through the wound. Now, in a patient of good habit of body, when the incisions have been properly conducted, the stone not large and readily extracted, and when the urethra and prostate gland have been divided by one cut, without hacking and mangling, there is reason to think the attempt might not unfrequently succeed, and thereby divest the operation of much of its terror and accompanying distress. It is only within these few years that my attention has been directed to this part of the subject; and I have already had the satisfaction of witnessing the wound made in the operation healed by the first intention in five instances, the urine passing off by the urethra from the commencement, and continuing to do so, whereby the cures were remarkably expedited. One of these cases was a child of 19 months, where the wound did not require a second dressing. In another of 3 years, the child was out, and going about within eight days after the operation. A third was 5 years; and the other two of 70 years. The two old men were each out of bed, and walking about the room, within three or four days after the operation.

Upon the principles now briefly stated, I have performed the lateral operation of lithotomy for thirty-six years, without, in one instance, declining to operate, however unpromising the circumstances may have appeared to myself or others.

A list of the cases, so far as I have been able to collect from my notes and books, is subjoined. How many more there may have been than what are there enumerated, I cannot now well say, from not having been in the habit, particularly in the earlier part of my practice, of taking notes, excepting where anything remarkable occurred. I recollect well, however, the distress of mind I experienced at the first case that proved fatal. It was the case of the boy J. D. October 1812. For the twenty years preceding that event, every patient on whom I had operated recovered; the impression on my mind then was, and the calculi in my collection confirmed it, that his was the thirtieth, although I cannot at present particularise more than twenty preceding him.

The result of these cases, I believe, may be justly adduced to demonstrate the main fact I wish to establish,—that the operation possesses in itself nothing intrinsically hazardous; that it is, all other circumstances considered, entitled to the character of being the safest and the easiest method of relieving patients of the disease; and that the circumstances that contraindicate its performance are greatly fewer than is generally imagined.

Name.	Age.	Place of Abode.	Date of Operation.	Event.
J. C.	5	Dundee	Feb. 1792.	Cured.
J. V.	40	Do.	March 1792.	Do.
W. L.	7	Do.	1793.	Do.
W. S.	4	Do.	1794.	Do.
D. G.	10	Do.	Nov. 1796.	Do.
J. L.	50	Strathmore	1797.	Do.
C. Do.	76	Dundee	April 1799.	Do.
J. C.	3	Forfar	1800.	Do.
R. M.	7	Crescent	March 1803.	Do.
T. W.	2	Glen Ogilvy	1804.	Do. (1)
J. D.	35	Dundee	Nov. 1805.	Do.
T. W.	3	Glen Ogilvy	1805.	Do. (2)
C. M.	60	Dundee	Aug. 1806.	Do.
J. M.	66	Do.	Nov. 1806.	Do.
Mrs ——	40	Do.	Nov. 1807.	Do. (3)
— Daught	15	Do.	1808.	Do. (4)
J. M.	66	Alyth	Dec. 1810.	Do.
J. B.	7	Lethem	1811.	Do. (5)
M. M.	48	Oathlaw	Oct. 1811.	Do.
C. C.	66	Dundee	Nov. 1811.	Do. (6)
J. D.	5	Do.	Oct. 1812.	(7)
J. B.	4	Cupar	Nov. 1813.	Cured (8)
J. C.	6	Do.	1813.	Do.
J. C.	19	Dundee	Dec. 1813.	Do.
J. B.	5	Cupar	April 1814.	Do.
C. Do.	66	Cortachy	July 1814.	(9)
G. S.	14	Dundee	Nov. 1814.	Do (10)
A. A.	50	Dunichen	Feb. 1816.	Do
W. L.	65	Dundee	March 1816.	Do (11)
D. S.	66	Do.	1816.	(12)
T. S.	60	Carmyllie	Sept. 1816.	Do. (13)
J. F.	69	Perthshire	Oct. 1816.	(14)
T. C.	70	Dundee	Dec. 1816.	Cured.

Name.	Age.	Place of Abode.	Date of Operation.	Event.
A. C.	60	Fairmuir	April 1817.	Cured.
G. H.	70	Inverighty	May 1817.	Do. (15)
P. B.	45	Errol	May 1817.	Do. (16)
P. C.	7	Dundee	Oct. 1817.	(17)
A. U.	59	Aberdeensh.	May 1818	Do.
D. T.	25	Perth	Sept. 1818.	Do. (18)
W. C.	66	Do.	March 1819.	Do.
J. B.	60	Dundee	Sept. 1819.	(19)
J. P.	17	Do.	Oct. 1819.	Cured.
W. W.	62	Forfar	Nov. 1819.	Do.
Rev. P. M.	56	Dundee	Dec. 1820.	Do.
J. A.	65	Muirdrum	April 1821.	(20)
R. Y.	60	Kirriemuir	June 1821.	Cur. (21)
W. B.	2½	Lochee	Sept. 1821.	Do. (22)
J. W.	53	Cortachy	Dec. 1821.	Do.
W. T.	60	Forfar	March 1822.	Do.
G. S.	2½	Dundee	April 1822.	Do.
Capt. K.	84	Do.	April 1822.	Do.
J. J.	72	Arbroath	July 1822.	Do.
J. P.	1½	Dundee	Aug. 1823.	Do. (23)
G. S.	4	Do.	Nov. 1823.	Do. (24)
Capt. K.	85	Do.	Nov. 1823.	Do. (25)
A. L.	67	Tealing	April 1824.	Do. (26)
W. P.	60	Foulis	July 1824.	Do. (27)
W. G.	70	Arbroath	July 1824.	(28)
J. R.	50	Perth	Aug. 1824.	Cur. (29)
Capt. M.	52	Alnwick	Aug. 1824.	Do. (30)
J. S.	73	Forfar	Nov. 1824.	Do.
Dr C.	78	Cupar	Nov. 1824.	(31)
J. D.	60	Tannadyce	March 1825	Cur. (32)
G. M.	65	Arbroath	May 1825.	(33)
D. H.	77	Dundee	July 1825.	Cur. (34)
A. L.	69	Tealing	Oct. 1825.	Do. (35)
J. B.	2½	Dundee	Oct. 1825.	Do. (36)
J. L.	70	Do.	Nov. 1825.	Do.
J. B.	65	Tealing	Nov. 1825.	Do (37)
J. S.	51	Dundee	Dec. 1825.	Do. (38)

(1.) Wound healed by the first intention. The child had been greatly exhausted from continued pain and irritation for several months. After the operation he soon fell fast asleep, and did not awake for twenty-four hours, excepting occasionally when voiding his urine, which he did altogether by the urethra. He suffered nothing, was very soon on his feet, and was taken home in perfect health within three weeks.

(2.) Was taken home quite well on the eighteenth day.

(3.) (4.) In both these cases the cure was most rapid. No incontinence of urine followed. The slitting up of the urethra was the work of a moment. I cannot but contrast these cases with the protracted agony I have latterly witnessed in the dilatation of the female urethra by means of Weiss's forceps.

(5.) Went home quite well on the eighteenth day.

(6.) Was walking about on the fourteenth day, and at his work on the twenty-first.

(7.) The parents of this child could never bring their minds to consent to an operation, until his life was despaired of. For several weeks he had had no remission of pain and straining. The pulse had become very rapid, accompanied with pain, tension, and tenderness of abdomen, flushing of the face, and cold extremities. The parents were in despair; and I yielded to their urgent entreaties to make an attempt to save the boy. The irritation never subsided; and he died under symptoms of abdominal inflammation.

(8.) Was taken home quite well on the eighteenth day.

(9.) Was brought to town in a cart, a distance of thirty miles, in a state of the greatest exhaustion from pain and irritation. The operation was put off from day to day, in the hope of the irritation subsiding; but the pain, irritation, and quickness of pulse continued to augment. He could take no sustenance, and his look was ghastly. He was not in a situation to be reconveyed home. The operation was undertaken; but his constitution never rallied, and he sunk away without pain about a fortnight afterwards.

(10.) Was sitting up within three or four days.

(11.) Took his food heartily from the first, and was going about on the eleventh day.

(12.) Had been in the habit of taking opium for many years to alleviate his sufferings. No desire for food, and continual thirst; constitution completely broken. A few hours after the operation fell into a state of complete stupor, accompanied with stertorous breathing. He recovered a little eighteen hours afterwards, but gradually sunk away.

(13.) Was out walking about on the fourteenth day.

(14.) Had been in the habit of taking laudanum latterly to the extent of four or five ounces a-day; had violent palpitations of the heart, accompanied with dyspnœa and occasional faintings. His case appeared very hopeless; but, yielding to his urgent entreaties, the operation was undertaken, from which he appeared to suffer little. He was up by the fireside next day, where he found himself more comfortable than in bed, on account of the dyspnœa. On the fourth day he fell into one of his fainting fits, out of which he did not recover.

(15.) About six hours after the operation I was sent for to see him; complaining of pain and tension of the lower part of the abdomen. Upon my arrival I found him quite easy, having just voided upwards of 1 lb. of urine by the urethra, not a drop escaping by the wound. He continued to pass his urine altogether by the urethra. Five days after the operation I found him sitting with his clothes on, and saying he found himself so stout, that he thought he could travel home.

(16.) Was brought into town in a bed suspended in a cart, in a state of the greatest emaciation and debility. Every five minutes he was straining to pass his urine, which seldom exceeded a teaspoonful at a time. He informed me he had been previously attended by three medical gentlemen, who gave him to understand that his complaint was a diseased state of the prostate gland, from which no relief could be obtained by an operation. Upon examination there appeared a great bulging into the rectum, obstructing the free egress of the fœces. A stone six ounces by weight was easily enough extracted. The prostate appeared in a natural state; the bulging into the rectum having been occasioned by the violent straining of the abdominal muscles forcing a lodgement for the stone behind the gland. Eight days afterwards he found himself so well as to be able to remove to a friend's house in the suburbs, for the benefit of better air and superior accommodation.

(17.) Had laboured under symptoms of calculus from his birth, till within the preceding four months, during which latter period he had been altogether free from pain, but his urine flowed off insensibly night and day as fast as secreted. Upon the introduction of the sound, a calculous concretion could be felt towards the left side of the fundus of the bladder, from which position it could not be moved, and from which it never varied. This, along with the cessation of pain, gave ground to suspect, that, during some of the violent paroxysms to which he had been subject, the stone had been partly forced betwixt the muscular fasciculi of the bladder, and had formed a cyst or pouch for itself in that quarter. The stone was laid hold of by the forceps, which, however, only brought off a portion. The fixed portion was dislodged with no small difficulty by means of the introduction of a female staff, and then extracted by the forceps. The cure went on without interruption, and he very soon regained the power of retaining his urine.

(18.) Was walking about the house on the thirteenth day; was out on the seventeenth perfectly cured. He had been affected from his earliest infancy; latterly his sufferings had been intolerable. The stone was studded with sharp points like the quills of a porcupine.

(19.) Had laboured under symptoms of calculus upwards of ten years, and about that period had made up his mind to submit to the operation; but after being laid upon the table, his resolution failed him, and he took to the use of spirituous liquors for relief. For the last four months, his sufferings had been without intermission, accompanied with a constant discharge of bloody, fetid, puriform matter, and occasional attacks of epilepsy. Becoming desperate from continued agony, and death staring him in the face, he peremptorily insisted upon being cut at all hazards. The stone was broken down in the extraction. The pieces, when put together, weighed upwards of eight ounces. He died apoplectic three days afterwards.

(20.) For seven days the cure went on delightfully. He was sleeping well, taking his food, and in excellent spirits, anticipating a speedy return to his family. On the evening of the eighth day, he was seized with a severe pain in his left heel, striking upwards with convulsive twitches. The spasms extended the following day towards the muscles of the neck, and he died tetanic three days afterwards.

(21.) Had also inguinal hernia on both sides.

(22.) Quite well within ten days nearly by the first intention.

(23.) Passed his urine altogether by the urethra from the first; wound never once looked at or dressed. Was running about playing himself quite well by the eighth day.

(24.) Passed his urine likewise by the urethra from the first. Perfect cure within ten days.

In both the above cases the urine was voided upon the introduction of the staff, so that they were both operated on with empty bladders.

(25.) Had been previously operated on eighteen months before, and had continued free of complaint for about twelve months, when he again began to complain. And it is something remarkable that four stones, weighing nearly four ounces, should have been formed in little more than as many months.

(26.) Cure by the first intention. Was up and had his bed made on the third day. Was walking about on the fifth, and was out quite well on the twelfth.

(27.) Along with symptoms of calculus, had been for many years affected with dyspnœa, so as often to prevent him from lying down in bed, with palpitation of the heart, cold extremities, &c. He was brought into our Infirmary; but his case appeared so very unpromising, that the surgeon then in attendance declined operating. Upon my accession to the charge, finding the poor man in such suffering from the calculus, I yielded to his entreaties; and what was very remarkable, within a few days after the operation, his pectoral complaints in a great measure left him; and he walked home, a distance of five miles, within four weeks afterwards.

(28.) Died the following day, under symptoms of great nervous irritation, with confusion of the head, and sinking at the heart.

(29.) Had been affected for twelve years with cough, dyspnœa, and puriform expectoration, as also frequent attacks of nephralgia, followed by bloody urine, passing of sand and small stones to the number of 274; and in addition to all these he had been for twelve months tormented with stone in the bladder. He went home two months after the operation, free of the calculous affection, though by no means free of his pectoral complaints.

(30.) This gentleman, who came all the way from Northumberland to put himself under my care, had been affected for twelve years with frequent attacks of nephralgia, followed by the passing of sand and small calculous concretions. For the preceding eight months had also been affected with symptoms of *calculus vesicæ*. The stone being supposed as yet small, it was proposed to employ Weiss's newly invented forceps, and extract the stone without cutting. But upon their introduction into the bladder, the smallest distension of the blades occasioned such excruciating torture, that the attempt was given up. Long-continued rigors succeeded, followed by high fever; and it was not till eight days afterwards that he was in a state for undergoing the operation.

He bore it with the greatest composure, and said, if ever it should again be necessary, he would not hesitate a moment;

but entreated to be preserved from Weiss's instruments of torture. Some weeks afterwards he embarked on board a King's cutter for Alnwick in perfect health; and what is remarkable enough, he has never since been affected with nephralgia or gravellish complaints..

(31.) Had lost the ability of one side from an attack of paralysis two years before. The stone weighed 6 oz., and was of a triangular shape, which rendered the extraction rather difficult. He promised well for the first twenty-four hours. On the second day great abdominal irritation took place, followed by symptoms of oppressed brain, which carried him off the following morning.

(32.) Along with symptoms of *calculus vesicæ*, had been subject for many years to frequent attacks of nephralgia, followed by the passing of sand and gravel. Since the operation these complaints have entirely left him.

(33.) Died under symptoms of abdominal inflammation.

(34.) Had likewise scrotal hernia on both sides.

(35.) Went home on the thirteenth day quite well.

(36.) Was up playing himself by the fireside three days after the operation. Healed nearly by the first intention.

(37.) Went home on the thirteenth day quite well.

(38.) Had a paralytic stroke some years before, leaving him disabled of the left side. Made an excellent recovery notwithstanding.

Dundee, 29th January 1828.

ART. II.—*List of Cases in which the Operation of Lithotomy was performed.* By ROBERT LISTON, Esq. Lecturer on Surgery, one of the Surgeons of the Royal Infirmary of Edinburgh, &c. &c. &c.

Name.	Age.	Abode.	Date of Operation.	Event.	No. of Calculi.
Rob. White	60	Kinross.	1821.	Cured	1
Peter Grant	79	Edinburgh	May 1823.	Do.	3
John Fletcher	60	Kirkaldy	June 1823.	Do.	5
Alex. Bain	74	Dalkeith	June 1823.	Died.	[a]
Mr R.	78	Annandale	July 1823.	Cured	1
James Craigie	70	Perth	Oct. 1823.	Do.	2
Geo. M'Donald	19	Dalkeith	Feb. 1824.	Do.	1

Name.	Age.	Abode.	Date of Operation.	Event.	No. of Calculi.
Sholto Douglas	15	Falkland	April 1824.	Do.	1
James Anderson	60	Edinburgh	July 1824.	Do.	2
Mr M.	58	Do.	Oct. 1824.	Do.	1
James Allan	3	Inveresk	Feb. 1825.	Do.	1
Mr M.		Edinburgh	Feb. 1825.	Do.	1 *b*
Alex. Leslie	65	Elgin	June 1825.	Do.	1
James Black	64	Edinburgh	June 1825.	Do.	1 *c*
Adam Kedgely	5	Do.	Sept. 1825.	Do.	1
J. Cuninghame	66	Torryburn	Sept. 1825.	Do.	36
Arch. Whyte	62	Edinburgh	Dec. 1825.	Do.	1
Lofty Murphy	19	Do.	May 1826.	Do.	1
George Roy	23	Do.	June 1826.	Do.	1
John Forbes	49	Glasgow	June 1826.	Do.	1
James Farquhar	81	Elgin	Aug. 1826.	Do.	1
Alex. M'Farlane	50	Leith	Nov. 1826.	Do.	1
Mr J. M.	56	Peebles	April 1827.		1 *d*
Mr B.	56	Caithness	April 1827.	Do.	1
David Lawson	50	Leith	Aug. 1827.	Do.	1
David Smart	2½	Edinburgh	Nov. 1827.	Do.	1
Peter Runciman	62	Leith	Nov. 1827.	Do.	1 *e*
Mr S.	19	Linton	Dec. 1827.	Do.	1
George Wilson	65	Leith	Jan. 1828.	Do.	2

(*a*.) This patient had laboured under the symptoms of stone for many years, and was much exhausted. His bladder contained a soft stone of considerable size, and above 100 small ones of various shapes. He died on the 10th day with gastro-enteritic symptoms.

(*b*.) This patient had a calculus of considerable size, but so brittle, that it gave way under the forceps. About five months after the operation, on account of a return of the symptoms, he was sounded again; and a calculus being discovered, he was cut on the right side of the perineum. A stone of the size of an almond was extracted, probably formed on a fragment of the first. The patient recovered rapidly from both operations. He died in 1827 from affection of the kidneys. Both of these organs were much diseased, softened, enlarged, and full of concretions. The bladder perfectly free from disease.

The occurrences in this case have contributed to render me more careful, when the calculus has broken into fragments, in examining the bladder before untying the patient. The stone should be carefully put together during the operation by an assistant, if the portions are not too minute, in order to ascertain that no fragment has been left in the bladder; and it has appeared to me proper in cases of this kind, whenever suppuration is fairly established, to examine the wound with a small and slightly curved sound, so that if any particles should still remain they may be removed.

(*c.*) A very large calculus was removed from this patient. His recovery was very favourable, with the exception of hemorrhage, from the wound and urethra, twelve days after the operation. This is an accident I have frequently observed, and is without doubt frequently attributable to venereal excitement. In young subjects, as noticed by Pelletan, it may occur from tenesmus, from worms, or other irritations of the lower bowels. *Black* recovered completely from the effects of the operation, but died nearly six weeks after from obstructed bowels, with vomiting, hiccough, &c. On dissection, an old and remarkably dense adhesion was found passing from the upper part of the mesentery to the rectum, separating the bowels into two portions. The stomach and upper part of the small intestines were distended and inflamed. The rest of the canal was empty and sound.

(*d.*) This is the only case in which I have had any trouble from hemorrhage, and it was necessary some hours after the operation to introduce some lint along the canula. The patient died suddenly apoplectic fourteen days after the operation. The urine had resumed its former course. The patient had repeatedly suffered from affection of the brain previously. This certainly cannot be reckoned a death from the operation.

(*e.*) This case, in which lithotritie was attempted, is detailed in the last number of the Journal. (p. 222.)

In a former number I gave a short account of the manner in which I have performed the Lateral operation. A sharp-pointed instrument has always been used for the incisions. These have been made on a large staff, with a wide groove betwixt its convex surface and side. I have never for show cut against time, but have very often finished the operation leisurely in less than one-and-a-half or two minutes. Seldom has a patient been above three or four minutes on the table. The only danger I have apprehended and guarded against (by the introduction of a large elastic tube) is infiltration. The greater number of my patients have never had a single bad symptom. I have generally looked upon them as safe whenever the urine came off clear by the tube. The only preparation I have ever adopted has been the exhibition of a small dose of castor oil the night before the operation, and even that may often be dispensed with. More is lost in the depression of spirits that attends delay than can ever be made up by physicking before, or attention after the operation. Pouteau seems to have been well aware of this fact, and gives some admirable cases in illustration.

ART. III.—*Observations on the best Method of accomplishing Delivery in Presentations of the Superior Extremities, where turning is unadvisable or impracticable.* By ROBERT LEE, M. D., Member of the Royal College of Physicians London, Physician Accoucheur to the British Lying-in-Hospital, &c. &c.

THE operation of turning in presentations of the superior extremities, though occasionally performed prior to the close of the sixteenth century, was not until that period introduced into midwifery as a general rule of practice. This important improvement, after meeting with much unreasonable opposition, was at last, and without having undergone a rigorous examination, implicitly adopted, and extensively abused. From the writings of the different authors on midwifery of the seventeenth century it appears that they considered the operation of turning as applicable to all cases without exception of arm presentations, and that to accomplish this object, they had recourse to a degree of violence, which must often have produced fatal contusion, or laceration of the structures of the mother.

Though such unwarrantable force as that employed by La Motte and his contemporaries, in altering the position of the child, is now less frequently exerted by enlightened practitioners, yet there can be no doubt that an equal degree of violence is still occasionally used, and is even inculcated by some of the most distinguished teachers in Europe, as justifiable and necessary, where turning cannot be more easily effected. * In this country at the present time, the opinion is very generally entertained, that we must change the position of the child, at whatever hazard, in all cases without exception, of arm presentations.

It is from a conviction that late writers on midwifery have not treated this subject with the degree of attention which its importance demands, and from having witnessed in several instances the fatal effects of injudicious attempts to turn the child in presentations of the superior extremities, that I have been led to a more careful examination of the circumstances which should influence our conduct in these cases, and of the best mode of accomplishing delivery where turning is unavoidable, or absolutely impracticable.

The chief object of turning, as appears to me, is the preservation of the life of the infant; and I must therefore say, that so serious an operation in regard to the mother is not justifiable, in cases where we have such positive proofs of the death of the fœtus, as want of pulsation in the umbilical cord, or the presence of unequivocal signs of putrefaction in the body of the child. I am of opinion also, that the operation is not to be resorted to, where we are certain that the pelvis is so deformed as not to admit of the passage of a living child at the full period, or where other circumstances present in the case render it very improbable that the life of the infant could be preserved, even if it were turned.

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* Douglas on Spontaneous Evolution of the Fœtus.

When we have recourse to turning, under the most favourable circumstances, as when the membranes are unruptured, the active contractions of the uterus feeble, or altogether absent, and its mouth dilatable, the operation is not even then altogether free from danger to the mother. I have seen it followed by severe and even fatal abdominal inflammation, though performed in the most cautious and dexterous manner.

Turning, then, whatever may be asserted to the contrary by some practitioners, being always more or less hazardous to the mother, it follows that her life should never be put in jeopardy by its adoption, where the child is dead, or where we cannot entertain a well-grounded expectation of saving the child's life by the operation.

But its difficulty and danger are greatly increased in cases where the *liquor amnii* has been evacuated for many hours, where the arm presents, and the uterus is contracting forcibly around the body of the child. I would ask, if, under these circumstances, it be not highly probable that the child's life has been destroyed by the great and long-continued pressure of the uterus around its body ; and it cannot be denied, that, in a large proportion of such cases, where delivery is effected by turning, the children are still-born ? In these cases, the danger resulting from not interfering, or from our interfering, if turning be the means we employ, is very great ; as rupture of the uterus in either case may be the consequence. If this state of violent contraction has existed for several days, as sometimes happens before our attendance is requested, the shoulder and chest have become so deeply and firmly impacted in the pelvis, that it has been found impossible to raise or move the child from its situation, so as to pass up the hand into the uterus to lay hold of the feet. The works of the best writers on midwifery contain cases of this description ; and Dr Denman has admitted, that, prior to the discovery of the process of spontaneous evolution of the fœtus, this impossibility of turning the child had, to the apprehension of writers and practitioners, left the woman without any hope of relief. The nature of this process is now perfectly understood. But I conceive that the interests of both mother and child, where the latter is ascertained to be alive, would be best consulted, by determining to place no reliance on a resource which nature so rarely exerts, and which, when exerted, can only produce the expulsion of a still-born child.

Blood-letting and opium, it must be admitted, have considerable effect in controlling inordinate uterine contraction, and thus facilitate the process of turning. But these means, even when largely employed, too often fail in producing that quiescent state of the uterus, which will enable us to effect our purpose with safety to the mother.

In some cases which I have witnessed, notwithstanding the employment of these remedies, I have found it impossible to pass up the hand beyond the presenting part into the uterus without exerting a degree of injurious violence.

Wherever, therefore, the valuable life of the mother is in danger, and no good can result from the operation of turning, so far as the child is concerned, I propose to do by art, that which is effected by nature, in cases of spontaneous evolution of the fœtus. I separate the arm from the body, perforate the thorax, and having fixed the crotchet on the pelvis or lower part of the spine, make such a degree of traction as may effect the delivery, without laceration or contusion of the soft parts of the mother.

In the following cases the above method was adopted with the most perfect success:—

On the 15th October 1824, I was called to visit a patient of the Westminster General Dispensary, residing in Great St Andrew Street. I found her in the following condition. The membranes had been ruptured fourteen hours, and the *liquor amnii* had entirely escaped. The right arm, much swollen and livid, was protruding out of the external parts, and the shoulder and a part of the thorax were firmly impacted in the pelvis, while the contractions of the uterus were violent and incessant. The pulse was quick, the face flushed, and the soft parts lining the pelvis were hot, dry, and very tender. Thirty ounces of blood were drawn from the arm, and sixty drops of laudanum administered, before any attempt was made to alter the position of the child. After waiting for half an hour, when the pains had somewhat diminished in violence, I attempted slowly to pass up my hand, but the pains were immediately renewed with redoubled force; and after persevering for upwards of an hour to turn, I was compelled to abandon the intention. Another practitioner then saw her, when other twenty ounces of blood were drawn from the arm, and forty drops of laudanum were administered. He waited some time, in the hope that the actions of the uterus would cease; but this not taking place, he proceeded to endeavour to pass up his hand into the womb. This attempt again excited the most violent bearing down pains; and after long and fruitless exertions, he also was compelled to desist from the threatened danger of rupture of the uterus.

Two hours having elapsed after this second attempt to turn, and the pains still continuing undiminished, I separated the arm from the body at the shoulder-joint, laid open the thorax by means of the crotchet, and, passing it through the opening thus made, fixed it on the lower part of the spine; and on dragging down with a steady force, the child passed out of the external parts doubled. Though there was great distention of the parts at the outlet of the pelvis, no laceration of these took place.

The superior aperture of the pelvis having been considerably under the ordinary dimensions, some resistance was offered to the passage of the head; but this was overcome without much difficulty. This patient speedily recovered, and has since been delivered by me of an eight-months child, where the breech presented, and where the life of the child was lost, from the time and force required to bring the head through the confined brim of the pelvis.

On the 1st May 1827, I was requested to visit Mrs Kagen, Charles's Street, Drury Lane, also a patient of the Westminster General Dispensary. She had been two days and nights in labour, and was extremely exhausted with fatigue. The left arm much swollen was presenting, and around it a loop of the umbilical cord, which did not pulsate. There was great thirst and restlessness, and the abdomen was tense and very painful on pressure. The pulse was extremely quick. The uterus was contracting with great force, and I found it quite impracticable to pass up the hand, or to push back the presenting part, so firmly was it impacted in the pelvis. Sixteen ounces of blood were drawn from the arm, and an opiate administered at 4 A.M. At 7 o'clock the pains had almost ceased, but were instantly renewed on attempting to turn. The child being dead, I did not persevere long in my efforts to turn, but delivered without much difficulty in the manner already described. Here also there was contraction of the brim of the pelvis, of which a lamentable proof existed in a fistulous opening between the bladder and vagina, reported to have been caused some years before by a protracted labour, which was terminated by the use of the forceps.

On the 14th May 1827, I was called to a patient of the same institution, in King Street, Drury Lane. The left arm presented, and the shoulder and thorax were forced deeply into the pelvis. The umbilical cord was hanging without the external parts, and did not pulsate. The contractions of the uterus were strong, and were much increased on attempting to introduce the hand. The delivery was accomplished with the utmost ease, and in a very short time, as already described. The extraction of the child was effected very slowly, to allow of the dilatation of the internal parts, and to prevent laceration of the perinæum. On the second day after delivery, this patient experienced a slight attack of abdominal inflammation, which readily yielded to one copious bleeding and cathartics.

In another case which has since occurred to me, and which, in all essential circumstances, resembled the three cases now detailed, the same method of accomplishing delivery was adopted, and with similar success.

I do not consider it necessary to attempt to define more clearly the cases to which the above practice ought to be applied, as it is hardly possible for any one, after the observations I have made, to misunderstand the object of this communication, or to suppose that the common operation of turning should be abandoned where there is a reasonable hope of saving the child's life, and that of the mother.

The method of effecting delivery above related, I was led to adopt, from reflecting on what takes place in cases of spontaneous evolution of the fœtus; and it may be perceived, that in all the foregoing instances nature had begun, and was striving, though ineffectually, to complete this process.

Since the occurrence of these cases, I have had an opportunity of perusing the essay of Dr Douglas on this subject, and have been gratified to find that he has recommended the same mode of treatment, and has been also forcibly impressed with the impropriety of turning in all cases of arm presentations. Dr Sims, in the 40th volume of the Medical and Physical Journal, stated similar views, but did not lay down any specific rule of practice in such embarrassing cases.

Dr Davis, in his Elements of Operative Midwifery, p. 326, concludes some observations on this subject with the following words. "If, therefore, we suppose the child to be already dead, or the circumstances of the labour to be such as to make it impracticable to bring it into the world alive by means of turning, or even to perform that important operation at all without exposing the mother to extreme danger, it would then, in my opinion, become the unquestionable duty of the practitioner to effect the delivery by embryotomy."

Instead of extracting the child double, he recommends "that it should be divided into two principal parts, head and body, by passing a properly adapted cutting instrument across, and through the entire structure of the neck;" and he has delineated in his valuable work instruments for this purpose.

Notwithstanding, however, his ingenious invention of Craniotomy forceps, and of the power which they confer upon us of extracting the head, or any other part of the child from the uterus, still I should be disposed to avoid, if possible, the occurrence of the head remaining in the cavity of the uterus after the extraction of the body, as it must be extremely difficult to find it afterwards for perforation, and quite impossible to accommodate it to the diameters of the pelvis in passing.

The difficulty of reaching the neck when the shoulder and thorax are thrust deep into the pelvis, and the head of the child is tilted up over its brim, appeared to me in the preceding cases so great as to be almost insuperable, setting aside the disagreeable process of passing up cutting instruments so high within the uterus.

December 26, 1827,
2, *Upper John Street, Golden Square.*

Art. IV.—*Observations relative to the questions, Is the Fibrin of the Blood of lower specific gravity than even the serum? Is the Blood which has a Buffy Coat after coagulation of lower specific gravity than Healthy Blood; and does it contain a larger proportion of Fibrin?* By John Davy, M. D., F. R. S., Physician to the Army.

Dr Scudamore, in his "Essay on the Blood," has answered all these questions in the affirmative. I shall take the liberty of considering them separately, and the propriety of the conclusions at which he has arrived.

1*st, Of the specific gravity of the fibrin of the blood.*

It seems to be a very natural conclusion to make, that the fibrin of the blood is the lightest ingredient of the blood, since it is the principal material of the buffy coat which often appears on the surface of the blood of persons labouring under inflammatory diseases: and that it is so, Dr Scudamore positively asserts. Thus, at page 35 of his essay, he says "Fibrin is lighter than the red particles, and also lighter than serum."

Yet, notwithstanding the appearance in favour of this assertion, the fact is otherwise;—fibrin is really denser than serum. The phenomenon of the buffy coat at the surface is deceptive, as is most easily proved by the simplest experiment. Thus, pour off the serum of the sizy blood, and cut off a portion of the buffy coat, and throw it into the serum; it will not swim, but invariably sink, if not buoyed up by air-bubbles.

When the nature of the crassamentum is considered, and the manner in which it forms, the apparent anomaly of the buffy coat being at the surface, though of greater specific gravity, will vanish.

In sizy blood, the serum and coagulable lymph are of greater tenuity, or less viscid, than in blood that is not sizy; and sizy blood is generally slower in coagulating than blood that is not sizy, though not universally so. When the former quality alone prevails, the buffy coat which forms is thin; when both qualities co-exist, the buffy coat is thick, from the greater depth of subsidence of the heavier red particles. Now, though the fibrin of the buffy coat is of greater specific gravity than serum, the *liquid* mixture of coagulable lymph and of serum is of less specific gravity than the liquid mixture of red particles, serum, and coagulable lymph, and in consequence the former becomes supernatant. This is not matter of conjecture; it is an almost obvious truth; and it may be proved to demonstration by a simple experiment. Thus, let the fibrin of the crassamentum be extracted by pressure in a linen bag, without the aid of water, and a liquid mixture of red particles and of serum will be obtained, on which fibrin floats, and the specific gravity of which I have found to vary from 1.070 to 1.075; and by the use of blotting-paper, to remove as much adhering serum as possible, it has been raised to 1.087.* Before coagulation takes place, it is this comparatively dense fluid which supports the liquid fibrin,—and when coagulation has occurred, the substratum of red particles enveloped in fibrin still gives support; and the longer the coagulation is in taking place, so much the less fibrin is mixed with the red particles, so much the more rises, and, *cæteris paribus*, so much the thicker the buffy coat becomes. In healthy blood, on the contrary, in which the serum and coagulable lymph are more viscid, the red particles are supported till coagulation takes place, and then they are retained like water in a sponge, and like water may be expressed from the sponge-like texture; or, if in excess, a part of them will separate and fall down, as the fibrin contracts.

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* This probably is very nearly the specific gravity of the red particles. After as much serum as possible has been abstracted by blotting-paper, the residue stains and penetrates the filtering paper, but not so as to drop from it. It is of the consistence of molasses; fluid to a certain extent, but not liquid,—a loose mass of spherical particles like a quicksand, or a bank of volcanic dust,—and it may help to afford some idea of that mysterious state of lava imagined by Dolomieu.

I have made many trials of the specific gravity of the fibrin of blood, and I have found it vary, (accordingly as it was more or less contracted,) from 1.046 to 1.057, and even 1.060. The first was the specific gravity of fibrin of healthy blood, or at least of blood that did not exhibit a buffy coat; the second was that of sizy blood, found in the right auricle of a man who had died of an acute disease, constituting what was formerly called a polypus; and the third was the specific gravity of fibrin of the buffy coat. Here, too, I find myself at variance with Dr Scudamore, who asserts that the fibrin of healthy blood is denser than that of sizy blood. If by density he means specific gravity, the facts I have mentioned are in opposition to his opinion. If he means any other quality by the expression, I must confess I do not comprehend his meaning.

His notion, that fibrin is lighter than serum, and the lightest ingredient of the blood, has led him into many mistakes, and given rise to many contradictions and inconsistencies in his essay, in which it would be equally useless to particularize, and irksome to criticise.

2dly, Of the comparative specific gravity of sizy blood.

In looking over the experimental part of Dr Scudamore's essay, I cannot find a single instance given of the specific gravity of sizy blood having been ascertained. He has come to the conclusion, however, that "blood which gives the fibrinous coat in a great degree, has a lower specific gravity than healthy blood;" but he presently adds, "that moderately sizy blood, drawn from a person before he becomes weakened, has not commonly a very low specific gravity."

The results of my experiments would seem to indicate, that there is no necessary, that is to say, no constant connection between the specific gravity of the blood and the presence or absence of the buffy coat. I have made many experiments on this subject. At present I can refer to notes of eleven only. In five instances in which the buffy coat was slight, the specific gravities were, 1.047, 1.051, 1.054, 1.055, and 1.054. In five instances in which the buffy coat was moderately thick, the specific gravities were 1.044, 1.038, 1.052, and 1.056; and in one instance in which it was thick, the specific gravity was 1.057.

It may be laid down, I believe, as a general rule, that the blood of persons labouring under acute disease differs in density very little from healthy blood, and is of comparatively high specific gravity, whether buffed or not; whilst, on the contrary, the blood of persons labouring under chronic disease, attended with debility and vomiting, is comparatively dilute and of low specific gravity; and this, though most frequently associated with the property of having a buffy coat, is not necessarily so.

Nor do I believe that the time required for coagulation is anywise constantly connected with the specific gravity, as Dr Scudamore asserts, who says, " blood possessing the highest specific gravity coagulates the most quickly;" and " that which gives the fibrinous coat in a great degree is slowest in coagulating." The blood, the specific gravity of which I found to be so low as 1.038, coagulated rapidly. On the contrary, I have known blood which differed but little from healthy blood, and which did not exhibit a buffy coat, remain liquid eight minutes after having been drawn, and not till the tenth minute was its incipient coagulation distinct. When the serum and coagulable lymph are not viscid, and, in consequence, not capable of supporting the red particles, these particles fall down rapidly; if the fluidity of the blood is great, two minutes are sufficient for their subsidence an eighth or even a quarter of an inch, as I have many times witnessed.

No doubt very buffy blood is very slow in coagulating, for the red globules cannot sink far in a very short time; and it is well known that healthy blood, which is of comparatively high specific gravity, coagulates rapidly. I wish merely to be understood to maintain, that the qualities in question are rather accidentally than necessarily connected, and that the general rule on which Dr Scudamore insists is not founded on a basis sufficiently broad.

3dly, Of the proportion of fibrin in buffy blood.

Dr Scudamore conceives that he has demonstrated by his experiments, that sizy blood contains a much larger proportion of fibrin than healthy blood, and that the proportion of fibrin is variable in different cups of blood filled at the same bloodletting.

The majority of his experiments on this subject I am under the necessity of remarking are far from satisfactory, in consequence of the proportion of fibrin not having been estimated for the *blood*, but for the *crassamentum*. As sizy blood commonly affords a contracted crassamentum, in which the fibrin is comparatively condensed, and in which the residual proportion of serum is comparatively small, it follows, that a definite quantity of sizy crassamentum *must* contain more fibrin than the same quantity of healthy crassamentum, and yet the healthy blood *may* contain more fibrin than the sizy blood.

In seven instances in which he notices the proportions of serum, and crassamentum, and fibrin, five were examples of healthy blood; and in these examples the largest proportion of fibrin per cent. in the blood was .27, the smallest .15, and the mean about .20. Of the two other instances one was of blood, "with a slight fibrinous coat," from a man having a slight pleurisy, which contained .36 per cent. dry fibrin. The other was an instance of blood with a fibrinous coat from a man ill of fever, which contained .26 per cent. of fibrin, a proportion not quite so great as the maximum in healthy blood. And soon after, in comparing arterial and venous blood, he states the proportion of fibrin in each of these as the same, and as high as .56 per cent. As he does not mention either of them being buffy, it may be taken for granted that neither of them exhibited this quality.

Nor does it appear to me that he has proved in a manner more satisfactory the other proposition, that the proportion of fibrin is variable, and sometimes greatly so in blood flowing in a continuous stream. His experiments relative to this point are deficient in proof, because he has not made the blood the subject of them, but the crassamentum, and has estimated the quantity of fibrin not for the former, but the latter; and the error to which this mode of proceeding is liable I have already pointed out.

Nor are the results of my own experiments in favour of the accuracy of these conclusions of Dr Scudamore, in the unlimited and strong sense in which he emphatically gives them.

The following table exhibits the results which I have by me on this question.

		Sp. Grav.	Dry Fibrin per cent.
1.	Healthy blood,	1.052	.15
2.	1st portion, slightly buffed,	1.054	.37
	2d, after loss of 24 oz. do.	1.053	.34
3.	1st, pretty strongly buffed,	1.044	.47
	2d, after 24 oz. less buffed,	1.042	.37
4.	1st, slightly buffed,	1.055	.13
	2d, after 2½ lbs. do.	1.054	.13
5.	1st, slightly buffed,	1.054	.17
	2d, after 24 oz. less buffed,	1.052	.16
6.	Buffy coat thick,	1.038	.34
7.	1st, moderately buffed,	1.058	.36
	2d, after 28 oz. strongly buffed,	1.057	.40
8.	Slightly buffed,	1.056	.40
9.	Moderately buffed,	1.052	.39
10.	Slightly buffed,	1.051	.41
11.	Do. buffed,	1.047	.36

In each instance, after the red particles were separated by pressure almost entirely in a linen bag, the residual fibrin was well washed, and dried at a temperature of about 212°, till it was brittle.*

The specific gravity of the blood was ascertained in the ordinary way, using a spherical bottle, with a portion of a thermometer-tube accurately fitted by grinding for a stopple. The blood was weighed after it had cooled; and the very minute quantity of air that entered in consequence of contraction from cooling was expelled by adding a drop of serum.†

From these results, it may be inferred, that there is no constant relation between the appearance of the buffy coat of the blood and the proportion of fibrin which the blood contains. No doubt, *cæteris paribus*, the buffy coat will be thickest when the proportion of fibrin is largest; and in most of the *phlegmasiæ* the proportion of this ingredient of the blood seems to increase as that of the red particles diminishes, but not, I conceive, generally to any very extent.

Farther, it would appear from these experiments that there is often a minute variation of the proportion of fibrin in the blood as it flows, judging from the quantity extracted from the first and last portions drawn. In all the instances of variation given, excepting one, the proportion of fibrin is smallest in the blood last drawn. In the solitary exception the increased proportion in the blood last drawn was distinctly ascertained. To what this peculiarity was owing is matter for conjecture. It seems to me very probable that it was derived from blood different from the general mass of the circulating fluid, and which had been liberated, as it were, from confinement, and restored to the circulation from a state of congestion in a diseased organ. The patient from whom this blood had been taken had been ill about twenty-four hours with dysenteric symptoms, complicated with pain in the region of the liver; and he was greatly relieved by the venesection.

Having now considered the questions proposed at the commencement, and assigned reasons for answering them in the negative, viz. that the fibrin of the blood is of higher, not lower specific gravity than healthy blood, and that it does not necessarily contain a large proportion of fibrin, I shall conclude, sedulously avoiding entering upon minute criticisms of the various inferences and reasonings connected with the conclusions which I have been under the necessity of opposing; and which, if the premises are proved to be erroneous, must themselves be incorrect.

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* I satisfied myself that by this process there was no loss of fibrin. The expressed fluid, thrown on a filter, passed through entirely, and afforded no residue.

† I may remark, by the by, that, were carbonic acid gas disengaged during the coagulation of the blood, it would not be an easy matter to ascertain the specific gravity of this fluid. Indeed, it would be almost impossible to do it with tolerable accuracy. But who ever complained of this difficulty?

On such a subject as the blood, so important in physiology—so important in pathology—and hardly less so, as it is commonly considered, in the practice of physic,—it is of consequence that we are not misled by " false facts," and from them deduce erroneous principles ; and the higher the authority, the greater far is the danger to be avoided.

Corfu, October 11, 1827.

ART. V.—*Observations relative to the question, " Is there any heat given off during the coagulation of the blood."*

By JOHN DAVY, M. D., F. R. S., Physician to the Army.

THIS question, it appears to me, has given rise to much more discussion than was necessary, and more perhaps than its importance deserves.

Mr Hunter, who first considered the subject, made an experiment, conducted in the most satisfactory manner, on the blood of the turtle, the temperature of which was the same as that of the air, and he could detect no evolution of heat in the act of coagulation.

I have made similar experiments with the blood of the turtle and shark, and with the same negative results.

Other experimenters have obtained different results, their experiments having been made amidst a complication of circumstances, so as to be of little value, and to baffle almost calculation. Dr Scudamore, I believe, is the last who has given himself the trouble to make these unsatisfactory experiments ; and he is of opinion, that, when blood coagulates, heat is evolved.

I shall take the liberty of considering the principal data from whence he draws this inference. They are contained in his " Essay on the Blood," published in 1824, from page 68 to page 84. He made three experiments, or rather I should say, repeated the same experiment thrice, under the same circumstances, on the coagulation of blood, using two thermometers, the bulb of one sunk nearly to the bottom, and that of the other kept about an inch below the surface.

At the commencement, in all the three experiments, the thermometers at the bottom and near the surface agreed. In the first experiment 92° was the temperature indicated, in the second 94°, and in the third 94°. But at the conclusion of the first experiment, after eight minutes, the upper thermometer was 90.25, the lower 90° ; in the second experiment, at the end of nine minutes, the upper thermometer was 92°, the lower 90; and in the third experiment, at the end of six minutes, the upper was 93°, the lower 94°. Thus, at the conclusion, in one instance, the upper thermometer is only one-fourth degree higher than the lower; in another instance it is 2° higher, and in the third it is 1° lower. Can confidence be placed in experiments in which there is such a want of harmony in the results?

Then consider the rate of increase and decrease of temperature in the second experiments and especially in the lower thermometer. From 94° in one minute it rises to 95°; during the two following minutes it is stationary; the next minute it falls to 94°; the next minute it is stationary; and the next it falls again one degree. Does not this call for the same remark as the preceding?

Dr Scudamore lays much stress on his fifth experiment, in which the blood was stationary for three minutes at 93°, and then suddenly rose to 94°.

After 5′ 30″ it was	93°
6′ 20″	92°
8′	92°
10′	90°
11′	89°
12′ 30″	88°

And, he remarks, "the blood was of jelly consistence in eight minutes, and firmly coagulated in ten." Here, then, the evolution of heat of 1° took place; five minutes before the blood was of jelly consistence; and during the interval of two minutes, viz. from 8′ to 10′, when it was firmly coagulating, it fell 2°. Does not this experiment, (were it deserving of confidence,) tend to prove, not what Dr Scudamore infers, but the contrary?

Dr Scudamore lays equal or more emphasis on the results of his next experiment on very sizy blood, in which the thermometer rose "from 80° to 81°, just at the moment that the concretion of the fibrin began. For a short time before it had been stationary, and afterwards it continued exactly at 80° for the space of five minutes." Why, I would ask, was it stationary before the concretion of the fibrin began; and why, immediately after it had risen 1°, did it fall 1°, and then remain stationary for five minutes?

These are the principal results which Dr Scudamore obtained, and of the accuracy of which he is quite satisfied, and from which he deduces the inference, "that the most remarkable evolution of sensible heat takes place just at the moment of time when the fibrin first concretes;" and that "a slight extrication of heat continues throughout the progress of coagulation."

On my mind, I must confess, in perusing the author's details, his results have made quite a different impression. They are deficient in uniformity; they are capricious and irregular, and indicate clearly either inaccurate observation, or, what is more likely, considering their nature, the interference of disturbing powers, (from which it is impossible to rid such experiments,) which entirely vitiate the results, and render them useless, or worse than useless, deceptive.

Mr Hunter's experiment was an *experimentum crucis.* It was a simple experiment. No disturbing circumstance interfered with its accuracy;—and *that* proved that there is no sensible evolution of heat on the occasion in question.

Moreover, abstractedly considered, ought we, *a priori*, to expect that any sensible heat should be produced during the process of coagulation? The coagulation takes place slowly. The greatest degree of contraction that the crassamentum experiences is many hours in being completed, and the quantity of matter that changes its form from liquid to solid is exceedingly minute. I have for this reason always thought, that, if heat be evolved during the operation, the quantity must be too inconsiderable to be detected. I might mention many facts in corroboration of this. I shall confine myself to one, which seems to me to be also an *experimentum crucis;* and, if considered so by others, this question will never again be agitated.

The fact to which I now allude is, that when the serum of blood is coagulated by means of dilute nitric acid, there is not the slightest elevation or change of temperature, as any one may satisfy himself by trying the experiment. It may not be amiss to give the particulars of one trial of this kind. I added an ounce of serum to an ounce of dilute nitric acid, each of the temperature of the air of the room, viz. 75°;—coagulation was immediately produced, but not the slightest change of temperature was occasioned. The mixture was exactly 75°. The dilute nitric acid used contained five parts of water to one of strong acid. The serum employed was of specific gravity 1.030; and, when slowly evaporated to dryness, an ounce of it afforded a brittle translucent residue, weighing 36.7 grains. It was procured from the blood of a patient labouring under a tedious chronic disease; which blood was only of specific gravity 1.047, and an ounce of it afforded 2.4 grains of dry fibrin.

Now, reasoning from these premises, must we not arrive at the conclusion anticipated, that no heat can be given off during the coagulation of blood, where so small a quantity of matter becomes solid, and that slowly, since none is given off during the coagulation of serum, when so large a quantity of matter is solidified, and that suddenly, and which in its nature is so analogous to fibrin.

I have only another remark to add. It should be kept in recollection, that there are many very striking exceptions to the rule of change of form, occasioning, or being associated with change of temperature; and that in every particular instance the question, whether the temperature will change or not change with change of form, can be determined only by experiment.

Corfu, Sept. 27, 1827.

Art. VI.—*Observations relative to the Question, "Is there any free Carbonic Acid in the Blood?"* By JOHN DAVY, M.D., F.R.S., Physician to the Army.

SIR EVERARD HOME and Dr Scudamore, the former in his Croonian Lecture for 1818, the latter in his "Essay on the Blood," published in 1824, have endeavoured to prove that blood, both arterial and venous, contains a notable quantity of carbonic acid gas, uncombined and free, a portion of which always separates when blood spontaneously coagulates, and the whole of which is disengaged when atmospheric pressure is removed by means of the air-pump.

In a note to a paper on pneumato-thorax, which had the honour of being published in the Philosophical Transactions for 1823, I have come to a contrary conclusion,—that blood gives off no carbonic acid gas under the circumstances specified, and that it contains none. My reasons for adopting this conclusion were the following:—

1*st*, Because the alkali in the blood is not saturated with carbonic acid.

2*dly*, Because the serum of blood is capable of absorbing carbonic acid gas to greater extent even than water, as I had ascertained by experiment.

3*dly*, Because during the coagulation of blood spontaneously, and the coagulation of serum by heat, I had never observed carbonic acid gas to be disengaged, when the experiments were properly made in vessels to which air could not have access, as in tubes completely filled with blood or serum, and inverted in blood or mercury.

Lastly, Because I had not been able to procure carbonic acid gas from blood just drawn from the blood-vessels, and still warm, when placed under a receiver, and completely exhausted of air.

The first reason which I have assigned must be universally admitted. It cannot be controverted. All those who have examined the blood are agreed that it contains free alkali, and that in a notable proportion. This then being granted, is it not contrary to all the rules of chemical science to infer, that blood should contain a free acid, even out of the body when destitute of life, and when undergoing changes in obedience to the laws of chemical attraction acting on dead matter?

The second reason is in harmony with the first, and a confirmation of it. Admitted as a fact (and it is most easily proved) that serum absorbs carbonic acid gas, and even more than water absorbs; would it not be wonderful indeed, if, with its capacity undiminished for this acid, it should give it off, and *that* while the blood is cooling and coagulating, and when it has the power of absorbing carbonic acid?

The third reason assigned is of a piece with the two preceding, and a still stronger confirmation of them;—I allude to the negative result of the coagulation of serum by heat. The experiment I have repeatedly made, and always with the same result. If, then, heat sufficient to coagulate serum does not extricate carbonic acid gas from it in the act of coagulation, can there be any free carbonic acid in the blood?

On the last reason which I have given, the negative result of no air being disengaged when fresh blood was put under a receiver and a vacuum formed, I shall not particularly insist; though it is conclusive as a negative result. Mr Brande's experiments quoted by Sir Everard Home, and Dr Scudamore's and Mr Gardner's experiments referred to in Dr Scudamore's Essay, already quoted, may be opposed to mine. Let the experiment be repeated by others and these accurate observers, and then the truth will appear. I shall lay no stress on the discrepancies of Mr Brande's results, and of Dr Scudamore's and Mr Gardner's, one obtaining, or at least stated by Sir Everard Home to have obtained, two cubic inches of carbonic acid gas from every ounce of blood; while the others obtained from this quantity of blood only about .04 cubic inch, or from six ounces rather less than half a cubic inch.

I now beg leave to give another reason in opposition to the statement of there being free carbonic acid gas in the blood. I have added carbonic acid gas to blood and to serum, over mercury, at the rate of one-fourth cubic inch of gas to the ounce, the whole of which has been absorbed, and yet the blood and serum still exhibited free alkali, indicated by the change produced on litmus and turmeric paper; and I have raised the temperature of the blood and serum to blood-heat, and no carbonic acid gas has been disengaged; and I have coagulated both by heat of about 200° Fahrenheit, and even then not a particle of gas was extricated.

If, then, blood and serum have the power of retaining at the temperature 200°, and in the act of coagulation carbonic acid gas added at the rate of one-fourth cubic inch to the ounce, and if this portion of carbonic acid is not sufficient to neutralize the free alkali of the blood, is it probable—is it possible, that blood can possess any free carbonic acid ?

On the datum which I have thus endeavoured to prove erroneous, Sir Everard Home has formed an hypothesis of the formation of blood-vessels ; and Dr Scudamore has offered an explanation of the coagulation of the blood.

Were the doctrine correct, it would be incumbent on the inquirer after physiological truth to scrutinize the speculations of these gentlemen, and endeavour to ascertain, if there is any necessary, that is to say constant connection between the cause supposed and the effect to be explained—between, in one instance, the passing of a globule of air through coagulated blood, and the production of new arteries, veins, and absorbents ; and, in the other instance, between the separation of an acid air and the coagulation of an alkaline fluid.

Corfu, Sept. 20, 1827.

Art. VII.—*Observations, Pathological and Practical, on Whitloe.* By David Craigie, M. D.

By many whitloe is regarded as a complaint too trifling to merit serious attention. When a person complains that he cannot use his hand because a finger is swelled and painful, he is recommended, in imitation of the older surgeons, to apply the balsam of Arcaeus, or Phioravant, or warm oil of turpentine ; to plunge it, after the manner of Homberg and Callisen, in boiling water ; perhaps to envelope it in a poultice ;—or, according to the compressive method of Parry, to apply a tight bandage. If, under this management, the pain and swelling go off, nothing more is heard of the complaint ; and the cure is ascribed to the powers of the turpentine or boiling water in suddenly arresting morbid action, to the emollient effects of the poultice,—or to the influence of compression in contracting enlarged vessels. If, however, the disease proceeds, and the skin giving way, unhealthy fungous granulations arise, with discharge of bloody ichorous matter, the red oxide ointment is applied partly as a stimulant, partly as an escharotic ; and the finger is allowed to recover or to become worse, according to the mildness or the severity of the disease.

These notions, which are prevalent not only among patients, but to a certain extent, if a judgment may be formed from what is not unfrequently observed, among surgeons, are somewhat favoured by the phenomena and effects of certain varieties of the disease. In others, however, they are entirely inapplicable; and if I am not mistaken, to this want of discrimination in the various forms which the disease assumes, may be traced much of the inert and undecided practice with which it is too often treated. The mild varieties may recover under almost any treatment; the severe are very generally equally unfortunate under all; and almost invariably terminate either in caries of the phalanges, or anchylosis of the digital articulations, so as to render the finger, if not the hand, nearly useless.

That this was not at all times the case I am well aware. The writings of Ambrose Paré, of Wiseman, of Gouey, of Garengeot, of Heister, of David, and many other surgeons, show that whitloe was always apprehended as a disease which was not only exceedingly severe at the time, but might terminate in the destruction of useful parts. I know not that I would be justified in asserting that the instructions and precepts of these authors are entirely overlooked by surgeons. But I think I have seen some reason to infer, that, if they are not altogether forgotten, they are by no means so thoroughly understood among the generality of surgeons, as to be steadily applied in practice. I may further be allowed to say, that if the apprehensions of the older surgeons, which were founded on an idea of malignant character and morbid acrimony, were carried in some instances to a vicious extreme;—the indifference manifested by several modern practitioners, from whatever causes it originates, is not much less reprehensible.

It is almost superfluous to say, that when any part of the finger becomes painful, swelled, and unusually hot, with or without redness, it is said to be affected by a disease which has been described by chirurgical authors under the names of whitloe, *panaritium*, *panaris*, and *paronychia*. There may be reason probably for inquiring whether every inflammatory disorder attacking the fingers is to be accounted whitloe;—whether it consist in inflammation modified by the texture and anatomical arrangement of the parts in which it occurs;—or whether it be disease of a peculiar kind, and the fingers are actually subject to inflammatory conditions of a different nature. I shall attempt, in the following observations, to examine this subject in such a manner as to elucidate, if not to resolve these questions, and to place in a clearer light the pathological relations of this disease.

This is not the place to inquire into the merits of all the divisions of the several forms of whitloe which have been at different times proposed by surgeons. When I mention that Gouey * admitted five species, that Dionis recognized only one,[*] Wiseman two[†], and that Garengeot admits four,[‡] while Heister refers them all to three,[‖] it is easy to see that they were not regulated by a uniform principle of arrangement.

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* Chirurgie Veritable, p. 307.

In general the French surgeons have followed the division adopted by Garengeot, which has the advantage of great simplicity in referring the four forms of the disease to the skin, the subcutaneous cellular tissue, the sheath of the tendon, and the vessels between the periosteum and the bone. From this the division of Heister differs only in converting the two first into one species;[§] while that of Callisen, the learned professor of Copenhagen,[¶] may be quoted as the counterpart of this, since he adopts the division of Garengeot, with the addition of another species, the *paronychia subunguis*;—by which he evidently refers to the disease described by Mr Wardrop under the name of *onychia maligna.*

The division of Wiseman, on the contrary, who referred the varieties of whitloe to two kinds, the *benign* or mild, seated in the skin, and the *malignant* or severe, affecting the ligaments and tendons, has been more or less generally adopted by the few English authors who have given the subject of whitloe any consideration. By Latta it is distinguished into two sorts, according as it is induced by an external cause, or comes on spontaneously;[**] while in the short and rather imperfect sketch of Benjamin Bell, the distinction is derived from the situation of the effused fluid.[††] By one ingenious author indeed, distinguished for the precision with which he writes, the division of Wiseman has been modified on a singular principle. Mr Pearson, in his Principles of Surgery, places the benign in the cellular tissue, thus distinguishing it from the cutaneous; while he enlarges the list thus formed by the addition of that which may on some rare occasions be traced to the operation of the syphilitic poison.[‡‡] This division has the disadvantage of being founded on two different principles, which coincide neither naturally nor accidentally. When one division is derived from the kind, severity, or degree of morbid action, it is not altogether accordant with the principles of correct arrangement to mingle with this another distinction derived from difference of affected tissue. With much less justice can the influence of a remote cause be admitted in the same line of distinction.

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[*] Cours d'Operations de Chirurgie. A Paris, 1714. Demontrées au Jardin Royale, par M. Dionis. 8 Demonstration, p. 602.

[†] Chirurgical Treatises, &c. Book i. chap. xi. London 1676.

[‡] Traité des Operations de Chirurgie, &c. Par R. J. Croissant de Garengeot Maitre, &c. A Paris, 1731. Tome iii. chap. viii.

[‖] D. Laurentii Heister Institutiones Chirurgicæ, &c. Pars. ii. Amstelaedami, 1739. Sectio vi. chap. clxx. p 1142.

[§] Ibid. Pars i. Sectio vi. chap. clxx. p. 1143.

[¶] Henrici Callisen, M. D. &c. Systema Chirurgiæ Hodiernæ. Hafniæ, 1815. Volumen I. Pars Altera, Classis i. Ordo iv. Genus iii. Species 10. Sect. 631.

[**] A Practical System of Surgery. By James Latta, Surgeon. Vol. III. Ch. iii. Sec. 8, p. 312. Edinburgh, 1795.

[††] A System of Surgery. By Benjamin Bell, Member, &c. Vol. I. Chap. ii. Sec. ix. p. 173. Edinburgh, 1801.

[‡‡] Principles of Surgery for the use of Chirurgical Students. By John Pearson, Surgeon to the Lock Hospital, &c. London, 1788. Chap. iv. Sec. i. p. 82.

To criticise the divisions of other surgeons, however, is not my object; and I have mentioned these modes of distinguishing the various forms of the disease, chiefly to show that the one originally proposed by Garengeot is superior to any of those which have since been given; and that surgical authors, in departing from this, have in general lost sight of the nature of the disease, and of the principles on which a just division should be attempted. The division of Garengeot, indeed, is liable to one objection, viz. that of superfluity, in representing one form of the disease to be seated between the periosteum and the bone. I trust that I shall show, before these observations are finished, that the disease is never primarily found in this situation, and that it passes, by a direct and easy transition, from the synovial membrane of the flexor tendons to the subjacent periosteum, which is then affected in a secondary manner only.

Every form assumed by whitloe, however varied in degree, in kind, or in extent, may be referred, I conceive, to the following three simple heads:—1*st*, That which is seated in the *corion*, or at least in its outer surface; 2*d*, That which affects the subcutaneous cellular tissue; and, 3*d*, That which, commencing in the synovial sheaths of the flexor tendons, passes not only to the cellular substance and skin, but to the subjacent and adhering periosteum of the phalanges.

I. The cutaneous whitloe, (*paronychia dermatica*,) is constantly limited to the skin of one, or at most of two of the fingers. It is almost never found on the skin of the hand; and it is generally restricted to one finger, or to the corion of one phalanx at a time. Though it may occur in persons of all ages and habits, the individuals particularly liable to it are in general adult, rather advanced in life, persons of meagre, asthenic, and anxious aspect, and pursuing occupations in which the fingers are more or less exposed to the operation of morbific agents.

It commences with a distressing sensation of painful searing heat, as if the finger had been scalded, referred to one phalanx, and not unfrequently with great accuracy to a single spot; while a slight streak of redness may be seen on some part of the skin. After this has been experienced for a space of time, varying in different individuals from ten to fifteen hours, considerable diffuse swelling has taken place, accompanied with much redness of the integuments. This discoloration, which gives place to pressure for a moment only, is seldom at the commencement accompanied with lustre; and it presents different shades as it extends along the finger. General heat and pain may occupy the whole finger, and even a portion of the hand. The swelling is confined to the finger; but the redness and the peculiar searing heat are limited to a definite space, which is generally marked by a distinctly circumscribed edge. As the disease proceeds, this edge advances regularly on the natural skin, but may be always recognized to be equally abrupt and circumscribed as at first.

When these phenomena have continued for the space of ten, twelve, or twenty hours, in some instances less, the cuticle begins gradually to be elevated, and to present a change of colour. It may be yellow or whitish, but most commonly assumes a tint bordering on purple, and at the same time becomes remarkable for its shining aspect. These appearances, which in general continue to extend from the spot originally affected over the whole finger, are occasioned by the interposition of a watery fluid betweed the *corion* and the *epidermis*, by which the latter is elevated from the former. This detachment generally takes place at first in small isolated patches, forming separate blebs or *phlyctœnœ.* But in the course of the disease, unopposed by remedial measures, the whole cuticle surrounding the finger may be detached; and in this manner the acute or active stage of the disease naturally terminates in the course of the third or fourth day, when it is not controlled by remedial interference.

The suffering occasioned by this form of whitloe is always more alarming than seriously injurious. It is accompanied through its whole course with so much pain and tenderness of the integuments, that the patient is utterly precluded from making any use of the finger; and even the hand sometimes cannot be moved. The slightest touch I have occasionally seen produce a most distressing sensation of faintness, from the effects of which recovery took place only after a considerable time. Sleep is always more or less prevented; the appetite is impaired; and the individual is distressed with a sense of restlessness and anxiety quite disproportionate to the cause on which it depends.

These symptoms are in some degree alleviated by the effusion of fluid beneath the cuticle; but they are never effectually removed until it is discharged by spontaneous rupture or artificial opening. When this is done it is found to be a turbid serum, sometimes yellow, generally of a purple colour, thin, but seldom transparent. The cuticle is dead; and the surface of the corion is covered with a reddish, flocculent substance, and semifluid, or of the consistence of thin jelly. From opening cases of cutaneous whitloe at different stages of progress, I am satisfied that this semifluid matter is the result of a process of partial coagulation of the fluid effused by the inflammation of the corion. Secreted at first in small quantity, it is very quickly separated spontaneously into serum, and a thicker coagulable matter. The former is the thin purplish fluid, which escapes when the cuticle is punctured. The latter is the thicker jelly-like matter, by which the cuticular surface of the corion is covered. Beneath it in general the surface of the corion proceeds, by secretion of proper epidermal matter, to form a new cuticle; and in a few days the original situation of the disease can be recognized only by the extent of the recent membrane, through which its tenuity allows the red colour of the corion to be distinctly seen.

In the preceding account I have been more solicitous to describe the progress and phenomena of this variety of whitloe from cases which have fallen under my own observation, than to trust to the scanty and sometimes imperfect notices to be found in authors, many of whom have allowed their observation to be misled by the descriptions of their predecessors. The cutaneous paronychia is represented by Garengeot as commencing sometimes at the root of the nail on one side, and spreading all round to the opposite corner,—" a circumstance from which," he adds, " it has received the name of *Tourniole* among the people." * This view, which I find is repeated by Mr Pearson, † of the situation and progress of the disease, though correct as to many instances of it, does not apply to all. In several cases of cutaneous whitloe which I have had occasion to see or to treat, the inflammation certainly spread from the point at which it first appeared, and seemed very often to creep round the finger; yet it also spread continuously over the whole skin of the finger, affecting not only the unguinal phalanx, but the middle, and occasionally part, if not the whole, of the metacarpal phalanx. This, it will be found, was the progress of the disease in a case which I shall very soon detail. From various cases, in short, which I have seen, I infer, that, though the description of Garengeot and Pearson is correct, so far as it goes, it presents too narrow a view of the disease. I doubt even whether its commencement is confined to the root of the nail; for I have seen several examples of it originating, so far as the patient's history could be understood or credited, in a different part of the finger. In such cases, however, it invariably spread more rapidly towards the nail than in the opposite direction towards the metacarpal region.

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* Traité des Operations de Chirurgie, Tom. iii. chap. viii. p. 286.

† Principles of Surgery, chap. iv. sect i. p. 83.

The pathology of cutaneous paronychia is to be understood by connecting the knowledge of the anatomical peculiarities of the digital corion with that of the phenomena which are observed in the course of the disease. So far as the skin of the fingers resembles that of other regions, it must manifest the same or similar living properties, and be liable to the same morbid conditions which are common to all other parts of the animal frame. The cutaneous texture of the fingers, for example, may be occupied with the same eruptions as are found to attack that of other parts. When the skin of the fingers is burnt, wounded, or torn, the same effects result as proceed from the same causes operating on other parts of the skin. So far the process of cutaneous whitloe differs not widely from inflammation of the tissue of the corion. The disease consists in inflammation of the outer or cuticular surface of the corion, spreading along that surface continuously, and producing the usual morbid products. In this respect, it resembles the genuine erysipelatous inflammation in other regions of the skin. Like that, it shows no tendency to confine its action to one or more points, to cause the secretion of coagulable lymph or proper purulent matter; but spreading in every direction along the corial surface, it causes it to secrete a serous fluid containing a small portion of albuminous matter.

The digital corion, however, presents certain peculiarities which impress a particular character on its morbid states. It is, in the first place, exceedingly vascular, more so than in any other part of the person, scarcely excepting the face. So far as can be judged by inspection, and the effects of mechanical means, it is much more dense and compact than in any other part of the body. Thirdly, Its proper sensibility, or what Bichat terms its organic sensibility, is very considerable; and that which it derives from the minute ramifications of nervous filaments is so great, that several physiological anatomists have ascribed to this circumstance the high sensibility and nice tact of the digital corion.

To these circumstances, which are well-known to anatomists, may be partly ascribed the readiness with which the digital corion assumes the peculiar inflammation occurring in whitloe. That it is in part only, I infer from the fact, which I have taken pains to verify more than once, that the paronychial inflammation, when affecting the skin, is considerably different from the pure erysipelatous inflammation, as exemplified in a scald of the same parts. In the former, the disposition to spread is conspicuous. It betrays also a greater tendency to sloughing, disorganization, and other marks of very unhealthy action. The action partakes very much of that which produces the phlyctænoid *bullae* of cutaneous gangrene; and indeed I have seen cases of cutaneous *paronychia*, which, but for their limited and local situation, presented every character of gangrenous *phlyctænæ*. The disease can further claim no essential character from its benign or malignant nature. From various circumstances it may not be severe; but it is, I am well assured, never entirely free from malignity.

Though cutaneous whitloe is not a very rare disease, in its genuine and unmixed character it is not frequent. Occasionally it is complicated, from causes which I am unable to explain, with the species next to be mentioned.

In the following case it was as pure and uncomplicated as I have ever seen it.

1. William Attershank, cooper, aged 54. June 6. On the middle finger of the left hand, the cuticle is entirely detached from the subjacent parts by the interposition of a watery fluid, from the root of the nail to beyond the articulation of the first and second phalangeal bones, over the whole posterior and lateral aspects of the finger. The cuticle is of a leaden colour, intermixed with purple patches, and seems to be much thickened. The cuticle immediately behind the nail of the ring-finger is in like manner detached.

The whole hand is affected with considerable diffuse swelling and redness; and the pain, which is constant and very severe, with a sense of scalding in the finger, extends up the arm, sometimes to the axilla. It has been less severe, however, since the cuticle began to change its colour. He can assign no cause for this complaint, unless it be occasioned by the introduction of a splinter of wood;—of which, however, he cannot speak with certainty.

The vesication was opened by the abscess lancet, and a quantity of reddish fluid discharged. He was ordered to apply an emollient poultice for one night.

On the following day, when the finger was much less painful, and the surface of the corion was seen suppurating, the dead cuticle was entirely removed, and the exposed surface was dressed with simple ointment.

Two days after a minute splinter of wood about two or three lines long, found impacted at the root of the nail, was removed by forceps; and the following day a new cuticle was seen forming on various parts of the injured surface.

This was an instance of cutaneous paronychia, originating apparently in the irritation of a mechanical agent. Yet it is easy to see, that, though this had some share in the production of the effect, its influence must have been small. In the first place, a splinter of wood is by no means invariably followed by whitloe; and it is probable that this man, as well as many others, must have had splinters run into the fingers repeatedly without being followed by such a consequence. Secondly, it is to be noticed, that not only the finger in which this foreign body was fixed, but the ring-finger also became paronychial. The latter appears to have been of the sort mentioned by Garengeot, as known among the common people in France under the name of *Tourniole.*

The appearance of this man was indicative of a deranged state of the general health. His complexion was dingy and sallow ; his look anxious and haggard ; his figure meagre and exhausted ; and his whole appearance bore the marks of extreme misery and distress. His digestive organs had been in disorder ; and steps were taken to correct them while under treatment. In other respects his case was favourable

The treatment of this variety of whitloe is simple, and may be comprised in few words. As soon as it is recognized, the cuticle should be freely divided by a lancet or sharp bistoury, or a *cornea* knife. The detached cuticle should then be removed entirely, or at least divided crucially, so as to allow the flaps to be reflected. After this has been done, it in general heals readily under the application of simple ointment, or the ordinary calamine cerate. If, however, it still betrays a tendency to spread, free removal of the detached epidermis, followed by the application of the resinous ointment, will rarely fail to effect a cure. In some instances early division of the skin by the scalpel is of much consequence in checking the progress of the disease.

II. The subcutaneous or cellular whitloe, (*paronychia subcutanea*, *parenchymatosa*, *paronychia telæ cellulosæ*,) or that seated in the cellular tissue is confined, in general, with very great accuracy to the pulp of one of the fingers. Whether it commences suddenly or slowly, it is easily distinguished by this circumstance, and by throbbing pain confined more particularly to a single spot, than in the cutaneous whitloe. Not unfrequently, however, especially when it spreads laterally, it is complicated with more or less corial affection, the cuticle rising in a semitransparent vesicle or bleb at one part, while the rest imitates the circumscribed swelling of inflammation of cellular tissue. General pain extends over the hand, and occasionally spreads up the arm.

When these symptoms have continued for a day or two, occasionally several, according to circumstances, the cuticle gives way by rupture at a single point in general ; and a small quantity of blood-stained serous fluid escapes. When it is next examined, a soft spongy brown red-coloured body is protruded through the opening in the cuticle, by the margin of which it appears to be almost strangled ; and by pressure a little blood-coloured fluid, mixed occasionally with particles of lymphy matter, may be expelled. If at this period the cuticle be removed, in the corion will be found an irregular opening containing a soft brownish-red granulation, and leading to the subcutaneous cellular tissue, in which an irregular cavity is thus formed. The pain, though with less throbbing, continues notwithstanding; and every attempt to examine the finger, or ascertain the extent of the disease, produces most acute suffering.

The duration of this form of the malady is influenced by several different circumstances of age, habit of the individual, exciting causes, and above all, by the mode of treatment.

In young subjects paronychia is comparatively a rare disease. In them the influence of morbific agents is less powerful than in persons more advanced in life, and whose mode of living is less calculated to resist the influence of disturbing causes. When, however, the paronychial action is developed in a young person, it almost invariably assumes the parenchymatous form; and its progress is then sufficiently quick. It is not improbable that this duration of the morbid process is occasioned by the peculiar susceptibility of the tissue which, under these circumstances, becomes the seat of the disease. I am not prepared to prove that the skin and the synovial sheaths are at an early period of life less liable to inflammatory action than the cellular texture of the fingers; nor would it be easy to discover on what properties, anatomical or physiological, such a peculiarity depends. From a considerable number of cases, nevertheless, of the different species of whitloe, I think I am justified in the inference, that every example of subcutaneous whitloe which we observe is found in persons under the age of 30 or 35. Several of the cases which I have seen or treated were in young women between 18 and 25; and I do not at present remember more than one instance of genuine subcutaneous whitloe in a person beyond 40. While I hazard this affirmation, however, as a general inference, it is not my intention to say that individuals under this age are exempt from the other kinds of whitloe. Before this period of life certainly they are rarely seen; but examples, especially of thecal paronychia, as I shall show, not unfrequently present themselves.

The subcutaneous whitloe, then, which is in a great degree peculiar to early life, is exceedingly rapid in progress. I have seen it terminate in resolution in forty-eight hours; and the space of five or six days is a common period in cases in which little or no matter is formed. The cuticle is then detached and thrown off dead. Even in the worst circumstances the duration of the disease seldom exceeds eight or ten days; for the patient then has recourse to surgical assistance, and by the use of appropriate remedies the career of the disease is arrested, or its progress to destruction of texture is at least moderated.

Previous to this time, indeed, a termination of the disease may be effected either by resolution or by the formation of more or less purulent matter; and as this is much influenced by the treatment, we occasionally meet with cases in which the symptoms continue for a much longer time, without tending decidedly to one event or another. It often happens that the symptoms of subcutaneous whitloe, either from constitutional peculiarity, or from the periodical alleviation of which the disease is naturally susceptible, cause so little inconvenience to the patient, that he continues to use the finger through the great part of the course of the disease. The throbbing pain is not constantly of the same degree of severity; and though a general sense of soreness and uneasiness in the finger and hand prevents the patient from forgetting his disorder entirely, the piercing severity of that in the pulp of the finger is liable to intervals of remission. Under these circumstances, some convenient domestic remedy is applied; and if this takes place, as is most natural, when the paronychial throbbing and piercing are at the worst, the slight alleviation which follows is naturally ascribed to the operation of the remedy. After this alleviation, the patient again proceeds to take liberties with the finger, till the return of the urgent tearing pain obliges him once more to try the effect of his remedy. In this manner the disease is in many instances alternately palliated and aggravated for days successively, without being brought to any positive termination; and it may continue for weeks even, until it is either, by adhering to some decided measure, permanently cured, or terminates in irregular suppuration and sloughing of the cellular membrane.

When it proceeds to this extent, an irregular sore is formed in the skin and cellular tissue, with much swelling of the surrounding phalanx. The sore is generally sinuous, with a narrow orifice, from which soft, fungous, unhealthy granulations protrude, with much pain, and a blood-coloured sanious discharge, or at least purulent matter, crude and imperfectly secreted. If, in this stage of the complaint, it is still neglected or treated inertly, the first result is in general the loss of the nail, with much pain and suffering; and the second is the extension of the disease to the periosteum of the unguinal phalanx, which then becomes carious, and causes a tedious train of complaints, unless the unsound bone is expelled by the process of nature, or removed seasonably by the interposition of art.

Subcutaneous whitloe, as thus described, is a much less formidable disease in the outset. Seated originally in the subcutaneous filamentous tissue, and partly in the adipose cushion, which forms the pulp of the finger, its pathological peculiarities depend chiefly on the anatomical and physical properties of these tissues. The adipose layer here is peculiarly dense, compact, and granular. Something also seems due to the tenseness of the skin of the fingers, which less easily yields to a distending force than in other parts of the body. This at least appears to be one reason why subcutaneous whitloe, if not resolved by prompt and judicious treatment, invariably produces more or less death of the cellular tissue, and not unfrequently of the corion itself. In this respect it is not dissimilar to the furuncular inflammation, from which, however, it differs in commencing, at least in the subcutaneous cellular and adipose tissue. Afterwards it certainly affects the corial tissue, and, if neglected, it passes in the reverse direction to the phalangeal periosteum.

Of the causes of this form of whitloe I confess I know little. Some cases I have traced to mechanical injury, a puncture with a needle or pin, or a splinter of wood, a bruise, and in some instances a lacerated wound. The proportion of cases, nevertheless, in which no circumstance of this kind can be recognized is so great, as abundantly to show that it is entitled to no other rank than that of an occasional exciting cause. In many instances I have seen it connected with symptoms of constitutional disorder. Several cases of subcutaneous paronychia which have fallen under my observation have occurred in young females, whose general health was delicate, and liable to frequent interruption from gastric, gastro-intestinal, or uterine irritation; and it is worthy of remark that the disease appeared to be as much under the control of the remedies ordered for the general disorder as of those employed for the local complaint.

The treatment of this disease is too often neglected till it has attained the suppurative, or rather the fungating stage. If it is taken at an early period, it may be cured with the greatest certainty by one good longitudinal incision through the skin and cellular membrane; and, if continued to the periosteum, it does no harm. The vessels of the parts are thus enabled effectually to empty themselves; the tension of the skin and cellular tissue are completely taken off; and the wound heals up, almost by adhesion, in a day or two, without any permanent inconvenience or deformity.

Many, I am aware, object to this practice as harsh and unnecessarily severe for a disease so slight as whitloe; and a poultice is too generally conceived the appropriate application. By others again the incision is condemned as irrational in the extreme, where there is no matter to be evacuated, and recommend it to be postponed till this consummation is effected. No reasoning can be more erroneous than that which dictates the treatment in this manner. It should be the duty of the surgeon to procure resolution, and to prevent the formation of matter; for if once this result takes place, the part which he has to perform in affording relief is that of a passive spectator only. In many instances of whitloe I never yet knew the poultice adequate to effect resolution, or to exercise any influence whatever on the disease, save that of relaxing the skin, and enabling it to be distended, or rather softened, with a very little less pain. The most powerful and certain resolvent, and the only one which I have found never to fail, is the longitudinal incision of sufficient depth, always understanding that it is not to be deferred till suppuration takes place. Though still useful, it then operates in a different way.

I am further aware that it is impossible to convince timorous patients of the necessity of the incision for the cure of whitloe; and in such circumstances a less formidable remedy is expedient. In one or two instances I have thought I succeeded in procuring resolution by the application of leeches to the affected part, and promoting bleeding by the emollient poultice. To this remedy, however, it is by no means safe to trust; and in the majority of cases it will at last come to the incision, when much less efficacious. The leeches, in short, may be recommended only in those cases in which John of Gaddesden found palatable and costly medicines expedient.

It is by no means, however, always the fault of the surgeon that resolution is not obtained. Many persons never think of applying for surgical advice till suppuration is either far advanced, or a bad and unmanageable sinuous sore, with fungous granulations, is established. The most effectual treatment in such circumstances I find to be division of the affected part by a deep longitudinal incision by the scalpel, so as to allow the application of local remedies to the seat of the disease. If to this purpose the longitudinal incision be inadequate, it should be crossed by a transverse one not less deep. After this it is of little moment what applications be used. The most generally suitable in the trials which I have made is the ordinary resinous ointment. When the process of sound granulation seems slow, the best remedy next to and after incision is the free application of the lunar caustic in substance. This is more especially indicated if the unguinal phalanx is felt rough and irregular by the probe. In one or two instances in which this was the result of neglected subcutaneous whitloe, after a free incision down to the bone, followed by the application of the resinous ointment, healthy action took place, and the granulations healed firmly over the bone. In other cases, however, the unguinal phalanx is so much injured that it must be removed.

It was my intention to illustrate the general principles above stated by the detail of several examples of this form of whitloe. I find, however, that this would exceed the limits within which this sketch must be confined; and I therefore mention briefly the following only.

2. January 21, 1817.—M. W., a young woman aged 22, has a large unshapely diffuse swelling affecting the unguinal and middle phalanges of the index finger of the right hand. The swelling is greatest in the site of the unguinal phalanx; and near the centre is a red prominent spongy mass, by the side of which the probe enters a short space obliquely through the skin and cellular membrane. The resistance of a bone is felt, but the surface is not rough. The sore discharges thin blood-coloured matter, and is the seat of much pain, which has been aggravated every night of late.

This succeeded an attack of much painful swelling, which first appeared about twelve days ago. Six days after the skin gave way; and in a few days more the spongy granulation appeared. She used poultices; but during the last three or four days she received from an apothecary some red precipitate ointment, with which she was ordered to dress it.

A deep longitudinal incision was made to-day through the skin and cellular membrane of half of the middle and the whole of the unguinal phalanx, passing through the sinuous opening; and a slip of charpée immersed in resinous ointment was inserted into the wound. The second day after, when the general swelling was much abated, it was dressed simply; and in three days more it was healed up.

3. January 1819. J. T., aged 28, servant. The unguinal phalanx of the middle finger is much swelled, and so painfully hot and throbbing, that she finds relief only from immersing it in ice-cold water. The cuticle is elevated like a blister; at one part dark-coloured, as if ecchymosed, and the pain and swelling at the margin of the nail is severe to an extreme. The finger first felt painful about five days ago, soon after a contusion with an axe.

A deep incision was made through the whole pulp of the finger, dividing the skin and cellular membrane. The wound bled freely. After immersion in tepid water, a slip of charpée dipped in resinous ointment was applied over the wound for two days. On the third it was dressed with adhesive plaster; and she returned after six days with the finger quite well.

4. John Johnstone, labourer, aged 50. October 21. The middle finger of the right hand is swollen, painful, and a little discoloured. On the palmar surface, corresponding to the middle of the unguinal phalanx, the cuticle is detached; and near the middle a single opening, which emits a discharge of sanguineo-purulent matter, leads the probe directly inwards to feel the resistance of the bone, but without encountering a denuded surface. The pain, which extends

over the whole hand and up the arm, though pretty constant, is intolerably severe during the night. In consequence of an injury received, as he thinks, five weeks ago in reaping, the finger began to be affected with swelling, painful tension, and much heat. These were generally so much aggravated during the night as to prevent sleep completely. Without any other remarkable occurrence, they went on till about three days ago, when they became less severe, in consequence of the spontaneous opening which previously took place.

A deep incision made passing through the opening, followed by the application of a poultice. He was also ordered an anodyne at night. In two days it was dressed lightly with resinous ointment; and in a few days more the wound healed up, leaving only slight stiffness and occasional pains. These were probably the effect of the long time during which the disease had been suffered to go on uncontrolled.

5. In October 1820 I was requested by a medical friend to look at the finger of a girl, whose name has escaped me, in the village of Morningside. It was a distinct case of subcutaneous paronychia; for the pain and swelling of which, affecting not only the finger but the whole hand, she had been under the necessity of quitting her situation. By the use of poultices which she had been applying sedulously for several days, the cuticle was loose, macerated, and whitish. But the pain was little abated, and the swelling was on the increase. The whole hand had a distressing sensation of painful weight, which obliged her to avoid using it, and sit by the fire with the arm suspended in a sling. No matter was yet formed.

I made along the middle and unguinal *phalanges* a deep longitudinal incision, from which a good deal of blood and some serous fluid flowed. After immersion in warm water, a slip of charpée, moistened with resinous ointment, was placed between the lips of the wound. In the course of eight days after, under the usual treatment, the finger was completely restored.

6. In the following case resolution appeared to be effected under the use of leeches. A young lady had painful throbbing swelling of the unguinal phalanx of the index finger of the right hand for eight or ten days. Poultices, Goulard extract, acetous and spirituous embrocations, had been alternately applied, with the effect of procuring temporary relief. But the painful sensations always recurred in a more aggravated form. When I saw it the finger was hot to the touch, red, tensely swelled, and so tender that it was difficult to make the necessary examinations.

I was desirous to employ incision immediately. But to this the patient was so reluctant, that I found it requisite to recommend, as the only alternative, the application of five leeches. Four were instantly applied, and drew a good deal of blood. The finger was afterwards immersed in warm water, and then enveloped in a poultice composed of the flowers of *Anthemis nobilis* well bruised. From this time the swelling subsided rapidly, the pain and heat went off more slowly, and the finger was finally restored.

Other cases of the same description which have since occurred to me, it would be too tedious to adduce. They illustrate the general principle which I wish to establish,—that the surgeon should never, if possible, allow the disease to proceed to suppuration; that he may always prevent it by seasonable incision; and that it is an erroneous principle to confine this measure to those cases only in which matter is formed. By the prompt employment of this method much painful suffering is avoided; time is saved; and the probable destruction of the nail and unguinal phalanx is prevented. The effect of the incision is to relieve tension and empty effectually the overloaded vessels.

III. The third form of whitloe, *(paronychia thecalis, p. synovialis,)* the felon or malignant whitloe of Wiseman, or that which is seated in the synovial sheath of the tendon, is not only the most painful and the most difficult to subdue, but is too often allowed to proceed to such an extent, before any active measure is taken to retard its progress, that the textures forming the delicate and complicated mechanism of the fingers are either seriously injured, or for all useful purposes irreparably destroyed.

Showing itself first in painful stiffness and diffuse tense swelling of the metacarpal and middle phalanges, it may either continue confined to these, or extend up along the lower surface of the palmar fascia, under the annular ligament of the wrist, by its tendinous arcades and sheaths, and beneath the fascia of the fore-arm to the neighbourhood of the *pronator radii quadratus.*

If it were of much practical importance to distinguish this form of whitloe, I should be inclined, from the cases which have fallen under my observation, to say, that, when confined to the finger, it is almost always an acute disease, proceeding with considerable rapidity to effusion of sero-purulent fluid; when affecting the palmar fascia it may be acute, but is often chronic; and when in the annular ligament, it is generally the effect of the disease of the finger being long neglected, or inertly treated.

In the tendinous sheaths its phenomena and progress are nearly the following. After a sense of throbbing pain and heat felt deep in the finger, and impeding its free motion, diffuse swelling, with more or less redness of the integuments, is observed. With various intermissions, especially of the pain, swelling continues, spreading over the hand and to the adjoining fingers, for three, four, or five days; after which one part of the finger generally becomes more prominent than the rest; and in two or three days more the finger has become so œdematous, that surgical interference is thought necessary. The skin does not often give way spontaneously; but the surgeon is too frequently tempted to wait, till under the use of poultices he conceives the matter is ripened. An incision is then made with a lancet, and a little sero-purulent fluid is discharged; and under the subsequent use of the poultice an ash-coloured piece of dead tendon makes its appearance at the opening. In this state the inflammation has effected death of the tendon, which eventually sloughs out by a long and tedious process; while, by adhesion and granulation within the sheath, its cavity is obliterated; the phalanges are either permanently incurvated, or rigidly and immoveably straight; and the use of the finger as an organ of prehension is utterly lost.

This may be represented as the simplest form of thecal *paronychia*, in which its action is confined chiefly to the sheath of the flexor tendons. It by no means rarely happens that the disease passes from the sheath to the subjacent and attached periosteum, when the inflammation established in this membrane speedily proceeds to caries, or death of the inclosed bone, with more or less unhealthy suppuration. This result, which is said by surgical authors to be most common in the unguinal phalanx, requires, however, some modification.

According to the original spot of the tendinous sheath which the inflammation affects, and according to the subsequent direction which it takes, it may affect either the articular capsule of the middle and unguinal phalanges, or it may affect, which I have more than once seen it do, the whole extent of the middle phalanx. In the former case it almost constantly produces caries of the contiguous articulating ends of the middle and unguinal phalanges. In the latter case, death of the whole middle phalanx is not uncommon. In other instances, however, partial death of part of the bone may be produced; and to this is then superadded caries, as an effort of the sound bone to rid itself of the dead portion. These states are easily recognized. One or more sinuous openings admit the probe to the bone, the surface of which is felt denuded, irregular, and grating; and not unfrequently the entire phalanx is more or less loose.

This last form of the disease has been described by Garengeot, by Heister, and others, as distinct from that which is seated originally in the synovial sheath of the flexor tendons. It is easy, however, for the anatomist to show, that inflammation commencing in the synovial sheath of these tendons, must, anatomically speaking, if not subsiding spontaneously, or checked by art, pass speedily to the periosteum of the phalanx, or the articular capsule, to which it intimately adheres. It is not my intention in this sketch to enter minutely into the details of the anatomical arrangement and characters of the textures composing the mechanism of the fingers. That has been already done by Camper with such accuracy and perspicuity, in allusion even to the very disease of which I now speak, as to render any description from me superfluous indeed.* I shall only recal, however, to the attention of the surgeon one or two points, which are necessary to maintain the simple principle for which I here contend.

The direction and insertion of the superficial and deep flexor tendons I presume to be well known. Their transition through a fibro-synovial canal attached to the *phalanges*, I also understand to be familiar. Let this sheath be divided longitudinally by the scissors; and its connection, formation, and structure, will be manifest. It will then be seen that it consists of two parts, one fibrous, opaque, exterior; the other synovial, transparent, and interior. The fibrous exterior layer, consisting of decussating fibres connected by cellular tissue, adheres to the lateral margins of the metacarpal and middle *phalanges*, and is lost, with the tendon of the deep flexor, on the upper half of the digital *phalanx*. This fibrous layer further, when traced carefully, is found to pass insensibly into the periosteum of the phalanges, and in truth may be said to adhere to it intimately. The transparent synovial or interior layer, which is exceedingly thin, and is most distinctly demonstrated by boiling, or immersion in hot water, lines this fibrous case above and laterally; but below, that is on the anterior or palmar surface of the phalanges, it lies in immediate contact with the phalangeal periosteum. Its connection with the articular capsules is too obvious to require particular notice. The effect indeed of the arrangement is such, that the entire synovial sheath is covered below by periosteum or articular capsule, and on the sides and above by a fibrous membrane. Altogether it may be represented as a *fibro-serous* or rather *fibro-synovial* membrane.

This synovial membrane, however, is not limited to the extent now mentioned. At the insertions respectively of the deep and the superficial flexor tendons, it may be traced from the fibrous layer backwards or upwards along the surface of the tendons themselves, and through the slits of the tendons of the superficial flexor. With some care this synovial investment may be traced upwards, at least as far as the tendons are lodged on the digital phalanges, when, corresponding to the lower extremity of the metacarpal bones, they are surrounded with the celluloso-adipose layer of the palm. It hence results, that on the palmar surface of each phalangeal row there is situate a synovial membrane, which, in the manner of a shut sac, not only lines the exterior fibrous sheath, but is reflected, in the language of the anatomist, over the contained organ.

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* Petri Camper, A. L. M. Phil. et Med. Doct. Demonstrationum Anatomico-Pathologicarum. Fol. Amstelaedami, 1760.

The tissue of the parts now described is the seat of this form of paronychia. Inflammation, originating in the synovial sheath, is speedily propagated, not only over its surface, but to the inclosing tissue, viz. the fibrous case above, and the phalangeal periosteum and articular capsules below. It is easy to see how readily it must, from this arrangement, pass to the tendons also, which, partly from this cause, chiefly from a degree of strangulation resulting from the diminution of the calibre of the cavity by the swelling of the synovial membrane, both of the sheath and of the tendon itself, are speedily killed. In short, the anatomical arrangement of the digital synovial sheaths explains at once the two principal phenomena for which thecal paronychia is distinguished,—death of the tendons, and caries of the phalanges, or their articular extremities.

The connection between these sheaths and the palmar fascia, and the annular ligament, shows the facility with which the transition of the inflammation to these latter parts may be effected. The palmar fascia, indeed, connected at its lower margin with the digital sheaths, and at its upper with the annular ligament, constitutes a very direct line of communication between the former and the latter; and, in point of fact, whitloe established in the finger speedily passes to the lower surface of the fascia, and in no long time to the synovial canals of the annular ligament. This, which was seen by Garengeot, by David, and by Sue, I have myself witnessed more than once; and it constitutes a variety of the disease, not only obstinate and troublesome to manage, but at all times severely injurious in its effects, attended with extreme pain and suffering, precluding sleep, and impairing the general strength. By passing to the articular capsules, it is often in danger of inducing caries of the carpal bones.

On the treatment of this form of whitloe some difference of opinion has prevailed; and, so far as I can discover, no very steady principle has been held in view in the several methods proposed. It was early recognized by Guy de Chauliac and John de Vigo as a malady painful and dangerous in the extreme, from the constitutional disorder which it causes, and even as occasionally fatal. In imitation of these surgeons, Paré recommended, with proper medical treatment, incision, deep and long, in order to give an outlet to poisonous and virulent matter.* Wiseman, aware that, if left to suppurate, the bone infallibly corrupts, or is affected by caries, with the view of giving the tumour what he terms a breathing, cut deep into the part most affected. * Both by Dionis † and Garengeot, ‡ the necessity of opening the sheath of the tendon by incision to evacuate the contained matter is inculcated; and the same measure was afterwards strongly impressed by M. David in his elaborate memoir on the Nature and Treatment of Abscesses, not only to give vent to matter, but to diminish the tension and strangulation of the membranes. § The justice of this doctrine has been in general admitted by surgeons. I shall show, however, that it is not conducted on the proper principle; or at least that the principle has not been understood.

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* Opera apud Uffenbach Thesaurum, p. 184.

It has been often observed that the incisions practised as recommended by Dionis, Garengeot, and David, were not followed by the subsidence of the disease and the restoration of the finger. On the contrary, it too often happened that after incision the disease seemed to proceed quite as violently as before, and in some instances to advance with increased rapidity and severity to destruction of the texture of the fingers. It was, therefore, argued that incision, if not bad, was at least not applicable to all cases of the disease, and that to the cure of many it was quite inadequate. M. David indeed himself had remarked that incisions which penetrate only to the seat of the sero-purulent or puriform matter, however long, are rarely sufficient to bring the disease to a favourable termination. The skin, cellular membrane, palmar aponeurosis, and tendinous sheaths, he remarks, are so swollen, that the traces of the incisions are soon lost. The parts rise above them, and conceal the cavity of the inflammatory process; and the purulent matter thus confined, he thought, becomes acrid and corrodes the bone. To prevent all these evils, therefore, he proposed, instead of merely dividing the sheath,—the operation then in use,—to remove by one stroke of the bistoury, down to the tendon or the bone even, a portion of the incumbent soft parts; and thus by a wide and extensive wound to give free vent to the thick, gray, curdy matter which the abscess contains. Thus he conceived the seat of the disease would be completely exposed to the access of remedial means; its further extension would be arrested; and caries of the phalanges so often observed was to be prevented. ||

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* Chirurgical Treatises by Richard Wiseman, book i. chap. xi. p. 56. Lond. 1676.

† Cours d'Operations de Chirurgie &c. par M. Dionis. Paris, 1714. 8tieme Demonstration, p. 603.

‡ Traité des Operations de Chirurgie, &c. Par R. Jacques Croissant de Garengeot. Tome 3ieme. A Paris, 1731. Chapit. viii. art. ii. p. 310.

§ Memoire sur les Absces. Par M. David.—Memoires sur les Sujets proposés pour le prix de l'Academie Royale de Chirurgie, Tome ix. A Paris, 1768.

|| Memoire sur les Absces chez Memoires pour le Prix, tome ix. section 3ieme, p. 260.

Whether this method really answered the object contemplated by its author I know not; for I have never met with surgeons in this country who practised it, and I never myself found occasion to do so. M. Sue states that its excellence is confirmed by the daily experience of practitioners in France.* M. David himself appears not to have put it to actual trial.

Another method of obviating the evils of thecal paronychia, without having recourse to incision at all, was proposed and practised by M. Foubert. This surgeon, distinguished for ingenuity and a thorough understanding of the principles of his art, was led by theoretical views, which this is not the place to explain, to think that escharotic substances presented an infallible means of checking the pernicious action of thecal paronychia; and with this view he applied, at an early stage of the disease, pastils of corrosive sublimate to the affected finger, and even on the extremity of the flexor tendons.† The method of Foubert, though imitated and defended by his pupil Fabre, seems nevertheless to have fallen into disuse, if not into complete oblivion; for in 1788 its merit was again discussed before the Academy of Surgery by M. Dubertrand in contrast with the method by incision, which was defended by M. de la Porte.‡

After comparing such cases as are recorded by the most eminent French surgeons with those which have come under my own observation, I think I am justified in the inference, that the true theory of the use of the incision is either overlooked or imperfectly understood. All of them proceed on the principle to which I have already adverted, that incision is premature while there is no proof of matter. It is conceived to be a measure not only cruel, but irrational, to augment the severity of a very painful disease by incisions, when there is no matter to be discharged. It is therefore said the surgeon must wait until he detect fluctuation; and then only should he use the knife.

This reasoning, which is founded on a loose analogy derived from the general rules of opening abscesses, is erroneous in the extreme. It is the delay till suppuration takes place that renders incision of little or no avail. The interior of the digital synovial sheaths is by no means similar to a portion of cellular membrane. They are fitted for containing the flexor tendons;

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* Reflexions et Observations Pratiques sur le Panaris. Par P. Sue Ainé. Memoires de la Societé Medicale d'Emulation. Seconde année. A Paris, An. vii. p. 303.

† Essai sur differens Points de Physiologie, &c. Par M. Fabre. A Paris, 1783. p. 157.

‡ Reflexions et Observations Pratiques sur le Panaris. Par P. Sue Aine. Memoires de la Societé, &c.

and the very circumstance which enables them to give these tendons support and resistance in the motions of the fingers, viz. their dense and unyielding character, prevents the sheaths from doing more than merely containing them. The moment inflammation commences in their cavity, as it spreads quickly over the whole, the swelling which ensues diminishes the cavity so much as not only not to contain easily the tendons there placed, but to induce quickly an extreme degree of compression. The unyielding nature of the fibrous case causes it to operate like a tight ligature over the whole course of the tendons. It thereby arrests mechanically the circulation within them; and this, as in other textures, speedily terminates in death of the tendon. This happens so invariably in thecal whitloe that the fact cannot, I conceive, be questioned. Perhaps the low degree of vital power which tendon is known to possess, may contribute somewhat to this effect. But that inflammation, as inflammation, is not the sole cause, I infer from the fact, that tendon is often inflamed in wounds and other injuries, without being completely killed. In short, it is a fact of which I have satisfied myself by examination of several cases of thecal paronychia, that, long before any suppuration takes place, more or less, sometimes the whole, of the flexor tendon is completely dead. It is, I conceive, no exaggeration to say, that ignorance of this fact is the reason why the surgeon waits until suppuration takes place, and thereby allows the season for checking the disease with effect to expire unimproved. Mortification of the tendons and extension of the disease to the palmar fascia and annular ligament, which is imagined to follow the incision, had taken place long before that event. Even before incision the periosteum is too often affected in sufficient time to produce caries, or even death of the enclosed phalanx.

For these evils the natural remedy, I conceive, is much earlier employment of the incision. To be efficient, indeed, the principle of this measure requires to be completely changed. Instead of using it with the view of opening an abscess and discharging purulent matter, it should be employed, first, as powerful and efficient means of local depletion; secondly, as the means of preventing that compression which, by its mechanical power only, must speedily arrest the circulation in the tendons, and thereby produce mortification of their texture.

It is chiefly from remarking the unfavourable terminations of cases of whitloe, treated on the rules of Dionis, Garengeot, David, and Pearson, left to the use of the poultice, or even neglected entirely, as often happens with patients who will not apply for professional assistance, until the disease is no longer tolerable, that I was led to employ incisions deep and long at an earlier period than is generally thought necessary. The effect was invariably more favourable; sometimes that of complete resolution. More than once, however, I have been interrogated on what principle I pursued this practice, and at first I was unable to give any other reply than the favourable result. The explanation above given is, I conceive, the true one; and if it be so, it demonstrates most indisputably the rule which I wish to establish, that not a moment should be lost in dividing, from the one end to the other, the tendinous sheath of the affected finger. It is almost superfluous to say, that the precept of Dionis of incision by the lancet, and that of Garengeot by the bistoury and grooved probe, are equally to be abandoned as inert and inadequate. The scalpel is the instrument which is to be used by the surgeon who wishes to subdue the disorder by one decisive measure.

I am not unaware that against this method, as against others, objections may be urged by those who view the disease through its symptoms only. Some, I dare say, may still ascribe the death of the tendon and the extension of the disease to the incision; and others may look upon the division of blood-vessels or even nerves as an objection. These I do not conceive it necessary to answer. I am satisfied from many facts, and the observation of wounds of tendons and tendinous sheaths, that no incision necessarily produces death in these textures. The division of blood-vessels is the best method of checking inflammation; and I am certain no hemorrhage from this source can ever cause inconvenience. If a vessel is partially wounded it should be divided entirely; and if that does not suffice it must be tied. This, however, I never had occasion to do but once; and I am disposed to think that a little time, with a piece of charpée, would have answered the purpose equally well. I may add, that this did not occur in the finger, but in the palm, where the anastomosing branches of the volar arch had been wounded. In the fingers the longitudinal incision, instead of dividing large vessels across, slits them longitudinally; and if bleeding from such a source goes to an extreme, the transverse division of the vessel or vessels furnishes a ready method of suppressing it.

Camper indeed has taken pains to remove all fears on this head, by showing that the nerves and vessels run by the side of the tendinous sheaths, and that the incision along the middle of the sheath can never be attended with danger. " *Scalpellus autem* mediam digiti partem dividere debet, vaginam, ligamenta, tendines compescentia ad os usque; neque nervorum aut vasorum laesione perterritus esse debet Chirurgus. Hallucinantur igitur qui a latere sectionem faciendam proponunt. Media pars tutissima est, quemadmodum exposuimus." *

After division of the sheath it is scarcely requisite to do more than dress it simply and lightly. The stimulating dressings of the old surgeons, as the balsam of Arcaeus, of Phioravant, or *du Commandeur de Perne*, are not requisite unless the tendons are partially or entirely killed; and indeed these hot balsamic applications are liable themselves to operate as escharotics, by inducing an extreme action in the vessels of the tendons. Much less is the proposal of M. David, of torrefying the surrounding parts by hot oil of turpentine, to be adopted. It is only when incision has been deferred until the tendons are mortified, that these stimulating applications may be used to facilitate the process of sloughing by ulceration and granulation; and to this the warm dressing (*linimentum terebinthinatum*) composed of turpentine and resinous ointment, or even resinous ointment alone, is amply sufficient.

The free division of the tendinous sheath will not, when deferred, be adequate to prevent mortification of the tendons; nor do patients always apply in time to enable the surgeon to put in practice this efficient measure. The moment, however, the disease is recognized, no time should be lost in waiting for suppuration, or on any other pretext. If the incision be not in time to save the tendon, and preserve the motions of the *phalanges*, it may at least arrest the progress of the disease to the annular ligament, and, above all, to the periosteum of the phalanges and their articular capsules, and to prevent caries of these bones. When this takes place, of course excision or amputation is the most speedy and effectual remedy; and perhaps in nine cases of ten it will be most expedient. In some instances, however, I have seen a free incision down to the bone, and dressing with resinous ointment, followed by consolidation of the parts. The carious portion of bone in such circumstances must have been small, and was probably removed not by exfoliation, but absorption.

These observations have extended to such a length, that I find it impossible to introduce all the cases which it was my intention to have done. The following, however, may illustrate some of the points which I wish to impress.

7. James Wilkie, labourer, aged 43. March 27. The middle finger of the left hand is considerably swelled, stiff, and extremely painful, with a leaden colour in some parts, a red in others, and a glistering lustre of the integuments. On the posterior surface the

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* Petri Camper A. L. M. Philosoph. ac Med. Doctoris, &c. &c. Demonstrationum Anatomico-Pathologicarum liber Primus, Continens Brachii Humani Fabricam et Morbos. Amstelædami, 1760. Tab. I. fig. ii. chap. i. p. 6.

fluctuation of matter is obvious. The whole hand, and each of the fingers, is affected with much swelling, in general of a white colour. This complaint began with pain in the middle finger, which had been previously contused, and exposed, as he thinks, to cold. He has applied nothing but poultices.

An incision made on the back of the finger discharged some blood and much matter, and discovered an extensive space of the metacarpal phalanx denuded. The next day the swelling was diminished, the leaden colour disappearing, and the general appearance improved. Another incision made on the palmar surface discovered the flexor tendons dead, and part of the metacarpal phalanx denuded. To the proposal of removing the finger the man would not submit; and deep and extensive incisions were made, and the warm dressings used. Under this treatment, the soft parts of the posterior surface first adhered by granulation with the bone. At the anterior the flexor tendons sloughed away, and union was at length effected without the loss of the bone. The motion of the finger was, however, lost, and the different phalanges remained permanently semiflexed. Some months after, when I saw this man quite well, he regretted that his finger, which was of no use, was not removed.

The following resembles some of the cases of Garengeot.

8. In a woman aged 65, the whole hand was swelled, tense, acutely painful, especially when touched, and the seat of a sensation of throbbing heat, most remarkable in the thumb. The painful tension and swelling were so general, that the motions of all the articulations were much impaired. She could assign no cause for these complaints, except the introduction of a small splinter of wood into the anterior part of the thumb near the last articulation. She applied nothing but poultices. An incision immediately made on the posterior surface of the thumb was followed by diminution of pain, and better nights.

Between this time, 14th April and the 20th, abscesses formed in various places, both in the palmar and antipalmar regions of the hand. They were opened by free and extensive incisions.

On the 1st May she had been restless during the night, with pain in the hand and wrist, which was much swelled with redness and tenderness of the skin. An incision discharged blood mixed with purulent matter; and a swelling which appeared the following day on the opposite side began to diminish. In ten days more the articulation at the thumb was found preternaturally moveable; and there was much pain and tension in the palm. A free incision made through the palmar fascia longitudinally was followed by complete alleviation of the pain and swelling there; but the articulating extremities of the phalanges of the thumb were rough and grating. The diseased bone was removed by amputation. The articular ends were found quite carious; and an ulcerated opening was found near the middle of the articulating surface of the last phalanx.

In the following case the disease was chiefly in the palm.

9. I was requested, on the 13th December 1819, by a medical friend, to see Mrs D., aged 69, whose hand had been swelled and painful nearly three weeks. The left was the seat of considerable diffuse swelling, great tension, and hardness; much pain, aggravated by pressure or motion of the fingers, with a sense of burning heat. The colour of the integuments was dull red, slightly disappearing on pressure, without lustre or œdema. Near the centre of the palm, upon careful examination, a soft elastic eminence following the long direction of the hand was felt. Several leech bites were seen in different parts; and a small circular purple spot, about one and-a-half line diameter, but without wound of the cuticle, was observed towards the metacarpal region of the ring-finger, where the pain was first felt. The fingers were retained in the semiflexed position; and any effort to move them was accompanied with extreme pain. The pain extends up the arm to the arm-pit. Pulse 110, full and strong. A puncture by a lancet was made last night without discharging any thing but blood. I instantly made a deep incision through the palm; and dividing the fascia to the extent of an inch and-a-half, about half an ounce of thick purulent matter, not uniform, but consisting of thick lymphy pus, floating in thin fluid, was discharged with a little blood. The relief was instantaneous. But in the course of a few minutes the patient fainted, and was some time in recovering sensation.

A poultice was applied for a day or two; and afterwards, under the use of the resinous ointment, and enlargement of the incision, with proper management of the general health, recovery was slowly effected in about ten or twelve days.

In the following case, which is well marked, and with inflamed lymphatics, resolution was at once effected by incision.

10. J. B., a young man of 23, applied to me in December 1817 for a complaint of the left hand, which had come on very suddenly. The middle finger was the seat of much swelling, tension, constant throbbing pain increased during the night, and redness of the integuments. From the finger a red streak ran up the back of the hand and the anconal surface of the radius, with an abrupt, though irregular and well-defined edge,—varying in breadth at different points, but broadest at the bend of the arm. Above this it becomes narrower, and ultimately terminates in a painful enlarged axillary gland. The lustre of the skin of this red tract is not more than that of the natural skin, rather less; and the redness marks exactly the boundary of pain, heat, and swelling, within which the skin presents a doughy sensation, quite different from that communicated by the sound skin. The motion of the elbow-joint, hand, and finger, is much impaired. The tongue is furred; the pulse 100, sharp and strong.

The finger was first affected with pain on Sunday (five days ago.) Yesterday the redness and pain of the arm came on, and was soon followed by affection of the wrist and gland.

An incision was made freely and deeply by a scalpel; and the wound was allowed to bleed, which it did most copiously. Next day the finger was better; and the second day after, the affection of the absorbents had disappeared. In a few days more the wound was entirely well.

This patient was also ordered to take a dose of a saline purgative.

Several similar cases, when presented in time, I have treated in the same manner, and with the same result. These it is unnecessary to detail, as they all of them present the same characters. The above I have selected as one that is at once well marked, and accompanied by affection of the absorbents. Upon the whole, I conceive it may be established as a correct principle, that in cases of thecal *paronychia* incision should be made at once through the sheath of the synovial cavity; and that this is the only effectual mode of arresting the disease, and of preventing those injurious consequences which too often result from the practice of waiting till suppuration is established.

ART. VIII.—*Case of Diffuse Inflammation of the Cellular Membrane from a Poisoned Wound, terminating favourably under the use of deep incisions.* By JOHN EVANS, M. D. Member of the Royal College of Surgeons of London.

MR LAYRD, surgeon of the Malin Dispensary, aged 26 years, living in a healthy part of country, surrounded by sea air, exercising freely, of regular habits, and in other respects possessing a good constitution, was called, on the 19th May 1827, to open an abscess in the scrotum of a poor man advanced in life, which was occasioned by retention of urine, whereby the urethra sloughed, and the urine was infiltrated into the scrotum and penis.

To give exit to this fluid, Mr Layrd made many punctures into the distended parts; and in making one of these, his right thumb, smeared with this sloughing matter, was pricked with a pin below the nail in the act of turning up the sleeve of the coat.

Acute pain, and more than what commonly attends a similar injury, was then experienced. The part was forthwith washed and well sucked. The person who gave rise to the accident died the following day. Mr Layrd now went to a neighbouring lake, and fished for some hours. He returned to dine at six, and in taking hold of a tumbler of warm liquor in the wounded hand, he, for the first time, felt an alarming sensation of pain and heat so pungent, that he almost dropped the glass on the table.

From this moment a train of severe complaints commenced;

nor can language sufficiently describe the sufferings of the unhappy individual. Up to the time of my first seeing Mr Layrd, I had the history and treatment of the affection, as well from himself as from Mr M'Dermott of Carn, who was most assiduous in his attendance on Mr Layrd.

It appears the pain, uneasiness, and burning sensation were confined to one spot, and this where the pin had entered, not being larger in extent than what a pea would cover, now became insupportable. He ran from place to place, scarified it and applied lunar caustic. Then he immersed the hand in warm water, when it became worse. This was changed for cold, but without alleviation. A warm poultice was next applied; but the pain increasing, this was laid aside, and adhesive plaster put on. Thus the night was spent in the most miserable way, without relief from any application.

20th. He was no better. A crucial incision was made over the thumb, and goulard put on. He was bled to sixteen ounces; but becoming weak when this quantity was abstracted, the vein was closed. Three grains of calomel, followed by sulphate of magnesia, produced five dejections.

P. M. 8th hour. I saw him for the first time twenty-six hours after the commencement of symptoms, and thirty-seven from the period of receiving the supposed poison. The pain indeed was now extreme. He had derived no benefit from the bleeding. Pulse 96, certainly one from its compressibility not demanding further depletion. He was in bed, tossing to and fro; his face flushed, as well from muscular exertion as from the warmth of the night and a confined room, from febrile reaction. It was distressing to see him gnashing his teeth, beating the bed with his feet, and calling out with the fiery sensation in his thumb. The blood taken was natural; nor was there any increased heat of body. Tongue dry, and brown in the centre, like that of a typhous patient on the third day of illness. The thumb now was red, swelled and painful; the burning sensation, which was constantly present, aggravated on the slightest motion or pressure of the part. He dreads approaching it. These inflammatory indications, with high morbid sensibility, are confined to the first joint of the thumb.

On examination, the lymphatics are seen running up the arm in two distinct lines on the inner side, to within three fingers' breadth of the axilla. The hand is now in cold water, often renewed; nor can he keep it one moment out of it. Three large leeches, all we had, were put on the wounded part. His calls were most urgent for the cold water during their application. The extracts of *Belladonna* and *Hyoscyamus* in watery solution were freely applied. On the sound skin, above the termination of the inflamed lymphatics, an eschar was formed circularly, by pencilling the part with nitrate of silver. He was ordered five grains of compound powder of ipecacuan, and one grain of sulphate of quinine, every second hour, with cold drinks.

3d day of illness.—The Dover's powder was reduced to half the quantity, the third dose causing retching. Some sleep, followed by frightful starting. Gently pressing the wound caused dark fluid blood to come away. The inside of thumb distended; the integuments tense; pain and uneasiness unabated. This part was now freely incised, till the knife reached the bone. Everything of yesterday continued. Urine voided in large quantity and clear. The inflamed state of the lymphatics stationary. He requested to have a trial made of warm water; but his sufferings were thereby increased, and the cold was resumed. Leeches were ordered from Derry, a distance of twenty miles; and, in the meantime, fit rollers were applied to the finger and arm. These were moistened by the narcotic solution spoken of above, and with as much pressure as could be borne. The parts were subjected to the pressure of rollers for two hours and-a-half. During this period, decided relief followed; but the leeches having arrived, the bandage was taken off, and twenty-two applied over the head and fore-arm with effect.

No relief followed this application, while the ease experienced during the pressure was succeeded by the former pain. Incisions extended; two others made on the thumb. Pulse 90. The pain before making the last incisions was truly frightful. He exclaimed in agony of the burning sensation in his thumb. The grumous blood continues to flow; no pus from any part; no motion. P. M.—Swelling decidedly advancing; it is now as high as the wrist.

Pil. Hyd. gr. vi. mane. Sul. Mag. et Infus. Sen. Pulv. Dover. et Sul. Quin. h. h.

4th day.—A better night. Changed the water often from hot to cold. The inflammation extending, a blush of redness is seen in the axilla; then the skin seems natural, while beyond this a second blush is seen on the side. Tongue dark and dry in the centre; pulse 83. Grumous blood still oozing from wound. The parts incised are less tense and painful. No shivering, headach, or unusual weakness. Intellect sound. Another incision on thumb. Gruel freely taken. Eleven leeches applied. Four natural dejections. After the leeches the pain became most excruciating. Dover's powder and quinine continued. A glass of port negus, with twenty drops of laudanum. Nourishment in abundance. A severe rigor in the evening. Extremities cold. Laudanum and negus repeated. Warmth applied.

H. S. Tinct. Opii. gutt. xl. Mist. camp. ℥*ss.*

5th day.—Two hours sleep. Symptoms aggravated. Pulse 96. Another incision on the back of the wrist. Lymphatics less red. He resumed the hot water. Took three ounces of wine at two times. No motion. Tongue as yesterday. H. S.—Has taken nourishment freely. Everything continued. *Tinct. Opii. gutt. lxxx.*

6th day.—Slept on the whole well; less pain; pulse 108. Epsom salts with effect. Two grains of quinine every second hour. No suppuration. The cuticle separated and removed from the thumb. The punctured part looks like half-boiled veal. The fore arm is much distended; and the pain in the evening intolerable. This part was freely laid open for five inches in length, dividing the tense fascia as it presented itself to the knife.

This incision was followed by great retraction of the integuments, and ease that cannot be described; but the pain next settled on the soft parts, covering the metacarpal bone of the fore finger. This was forthwith divided with the usual relief. The finger could be insinuated under the divided edges of the integuments, and these readily everted. It seemed as if they had lost their adhesive connection. The countenance and features look sharp and haggard. He is restless and peevish; urine clear and voided in large quantity. Everything ordered taken. Nourishment freely, consisting of mutton-chop, soups, &c. &c. He lies now with his arm and shoulder in hot water. He is propped by pillows half out of bed; and the whole extremity, up to the shoulder, is in a bullock's feeding-trough.

P. M.—Pulse 120. No improvement in the tongue. Severe paroxysms of pain running in the direction of the radial artery. At six Dr Rogan arrived, and a consultation was held with this gentleman and Mr Irvine, R. N. The result was to follow up the practice of incision as heretofore; and whenever pain and tension become urgent, then to use the knife without reserve.

About this period a singular nervous sensation manifested itself. It was like deep and weighty sighing, referred to the præcordia, which no effort of his could overcome or restrain. The inspirations were short and with a loud noise, while the expirations were long and anxious. Two ounces of port. Laudanum repeated to eighty drops. It was applied also to the pit of stomach. The former remedies continued.

7th day.—Some sleep in the night, and two hours freedom from pain after laudanum. After an interval of six hours it was repeated to seventy drops. The pulse and tongue as last reported. Affection of præcordia distressing. An old incision extended. At consultation it was resolved to give laudanum steadily, seventy drops every sixth hour. If the pain remains unmitigated the interval to be four hours. Sherry, one pint in twenty-four hours. Nourishment to be continued largely, paying attention to the stomach.

Laudanum taken at 10 and at 4 P. M. Wandering in intellect; inflammation extending. At 8 the laudanum omitted, and the following substituted by Mr Irvine.

℞ *Ext. Hyoscyam. n. gr. viij. Pulv. Camphor gr. iijss. formâ boli. Repetatur T. Opii gutt. xl.*

Two incisions made over the radius and ulna were followed by immediate ease.

8th day.—Hot applications continued. He had an hour of refreshing sleep, and awoke covered with perspiration; urine copious; the inflammation extending; pulse 106. Three natural fluid dejections. Wandering of mind continues, but is collected when spoken to. He still has deep inspirations, with soreness in the epigastric region; and feels as if there was spasm in the cardiac portion of stomach. The skin is natural. Quinine continued; laudanum at the hour of rest.

9th day.—Every symptom aggravated; bowels often moved; pulse 120. He complains of pain in the direction of the œsophagus going into stomach; difficulty in swallowing. He has taken 160 drops of laudanum in three doses. Camphorated liniment to the throat and neck.

Yesterday he was resigned and courted death; at present he is more cheerful and anxious to live. The medicines were continued. P. M. Nourishment abundantly taken; six fluid dejections. Eight ounces of Madeira consumed with malt liquor occasionally. A former incision deepened from a sense of fluctuation, was followed by blood only. He appears rapidly sinking, as well from disease as from diarrhœa. ℞ *Acetat. Plumb. gr. vij. Pulv. Opii, gr. iv. In Pil. h. s. iv. unam 3tia hora.* An enema of starch with 100 drops of laudanum. This to be repeated in four hours. Omitting the laudanum; the quinine continued.

10th and 11th days.—Pulse 106–20; inflammation above the elbow. The laudanum in full doses at night. Sleep. A bottle of Madeira daily, with as much malt liquor. The arm is now frightful; phlyctenæ scattered over it, and a large one on the fore finger; urine still clear and voided in large quantity.

12th day.—Inflammation over the shoulder. The hot water discontinued, and a flax-seed poultice applied. Diarrhœa has ceased.

13th day.—The quinine was continued steadily at the rate of two grains every second hour till this morning, when he refused every thing in the shape of medicine. The diet, consisting of the most nutritious articles, given often, and retained in large quantity, with it appears *nearly* two bottles of old Madeira in the last twenty-four hours, independent of London porter or ale, which certainly exceeded another bottle, has been duly persevered in. The wine is now restricted to half the quantity, with some ale. Pulse 116; the tongue as unfavourable as it can be. Marks of inflammation are now seen on the side; it is of a bright scarlet colour, but the tension seems less; some discharge of a puriform nature from original wound. P. M. Quinine resumed in grain doses every second hour, and the laudanum at night. The tongue moist at the edges.

18th day.—Discharge purulent from thumb, sparingly so from one of the incisions; the extremity, enormously swelled, is in girth nearly equal to a moderately sized pillow. The redness is now becoming pale; fluctuation evident through the whole arm, but the boggy feel only at the inner margin of the deltoid.

An incision was made near the origin of the *supinator radii longus*, and a large quantity of pus came off, by no means unhealthy. Another opening was made along the lower portion of the *flexor carpi ulnaris* with the like result. It was opened after this by Mr Irvine at the part reported boggy, and matter evacuated. The greatest relief followed these several incisions. The tension became less; the arm more manageable, and diminished in size. Before this, moving or changing its position, from its weight and pain, was a task of the greatest difficulty. Tongue assuming a natural appearance. Every thing continued. Wine reduced to half a bottle in the day. Pulse 104.

19th day.—The openings continue to discharge; no sloughing of cellular membrane; the exhaustion and emaciation great. He has had severe pain in the opposite side, accompanied with much difficulty of breathing, amounting to a kind of paroxysm, and seeming to affect him so violently that he could not survive it. This continued for two hours. I saw him when at its height. Hot bricks were applied with relief. In the originally affected side, at the inferior angle of the scapula, pain and uneasiness were complained of. It was examined, and matter was discovered on the inner edge of the *latissimus dorsi*. Here again the boggy feel was well recognized. Below this abscess the integuments seemed to present a contracted or adhesive feel, defining its lower portion. We found it did not extend beyond this subsequently. A free incision was made into this part, and sixteen ounces of matter taken away. It was left discharging.

In this matter, were seen large portions of detached cellular membrane, and clots of dark blood. Wine, diet, and quinine continued. Laudanum at night.

25th day.—Continues to improve.

July 12th, 55th day.—Every thing favourable. He was out of doors a few minutes for the first time to-day. The original wound still discharging purulent matter; that from the side is of a serous nature. No power of moving or using the extremity. He is still very much emaciated.

It is superfluous to refer to the many valuable cases published on similar affections. That now detailed justifies the opinion that neither any bad habit of body, or unwholesome condition of atmosphere, or the injury itself, could have availed any thing in inducing a disease so severe; and that all was caused by the minutest portion of an animal poison entering the system, and this poison generated during the life of the individual who communicated it. Similar occurrences do not often happen. Let the nature of the poison be what it may, it derived its virulence from extravasated urine, causing gangrene of the parts in contact with it, and involving extensively the cellular membrane. That the person imparting this poison was free from any disease till attacked with the retention of urine, may be inferred from his general healthy condition and his country life.

As caustic was early applied, and the part also early scarified, we are told a portion of it cut out, we have no knowledge whether a pustule or vesicle had formed. But the inflammation being continuous, the numerous *phlyctænæ* on the arm and forearm, and the absence of glandular inflammation, prove the severity of the disease, and the danger of the patient, while they strengthen the belief of an animal poison giving rise to the whole train of symptoms. We need hardly confess our obligations to Mr Travers for his judicious remarks on constitutional irritation. Nobody will doubt there was in Mr Layrd's case what this gentleman calls "prostration with excitement." The sighing, sinking, the indescribable uneasiness about the præcordia; the affection of breathing, with uneasy sensations in the diaphragm on the sixth day; the difficulty of swallowing; the jactitation; the state of the pulse and tongue, and the thirst, up to the formation of pus, and its discharge, all tend to establish this belief.

Throughout the course of the affection the urine was clear, and was noted by the attendants to be unusually abundant. The absence of morbid heat is equally strange. With respect to the treatment we have only to state, that without the knife, used both freely and extensively, every effort to save life would have been unavailing.

The incisions were made, in the first place, to relieve the insupportable tension and distension of the fascia, and the soft parts exterior to this, thereby freeing the sufferer from the pain attending thereon, while the amount of pain induced him to acquiesce in each operation, as ease in every instance followed. It is the opinion of his medical attendants that these states of pain, tension, and distension, would, by their acting on the nervous and cerebral systems, have destroyed him long ere matter could have formed. But the incisions were resorted to, in the second place, to give exit to accumulated collections of matter; as in no case, save at the thumb, where the poison was first introduced, did it find a passage by the first formed ones. The cellular membrane presented a whitish appearance at each incision; afterwards it looked like rotten tow.

Thus it seemed the superficial fascia, with the soft parts exterior to this, was in the beginning the seat of the affection; while the formation of pus was confined to that covering the muscles.

Those who advocate the depleting system in the case before us, will be disappointed when they perceive that neither the general or local abstraction of blood availed any thing. I do not say that the former rendered the disorder more untractable, yet I confess my doubts of its having been of use; while the latter certainly neither mitigated pain nor assisted in arresting the progress of the malady; and it is the conviction of the sufferer himself, that in both applications the leeches did harm.

I am of opinion that the constitution was affected, first, through the agency of the nerves; then we traced the poison to the lymphatic system; and lastly, continuous inflammation, the "specific inflammation peculiar to poison," the "erythematic" of Mr Travers, pervading the cellular membrane and blood-vessels.

The grumous blood coming from the wound for so long a time appears to have been the effect of the poison, acting on the vessels locally. The inward nervous sensations, as sighing, sinking, &c. were in all probability caused by this poison acting on these systems circulating through the blood-vessels. Throughout the course of ailment the glands in the axilla remained unaffected. We need hardly say that the applications to the extremity were changed according to the feelings of the patient; while the cordial and nutritive mode of treatment was persevered in to support the system, struggling with the effects of a malignant poison.

October 13th.—Mr Layrd has not recovered the use of the extremity. He is unable to perform almost any of the common duties with it. His health I believe to be otherwise good.

ART. IX.—*Case of Amputation of the Thigh during the progress of Gangrenous Inflammation.* By GEORGE MACDERMOTT, Esq. Surgeon of the 4th (King's Own) Regiment.

PRIVATE JOHN STOKES, 4th (the King's Own) regiment, aged 22 years, tall and slender, of a florid complexion, was a young man of sober habits, and generally healthy. While the regiment was quartered at Lima, in Portugal, he fell on the 16th May 1827, at eleven A. M., from the window of a convent upwards of thirty feet high, dislocated his right shoulder, and fractured both bones of the right leg, about four inches above the ankle-joint. The fracture was compound; but the wound in the integuments was very small, and just over the spine of the tibia.

The man was immediately brought to the hospital, where I saw him a few minutes after the accident. He was then pale and exhausted from the combined effects of fright and injury. I reduced the dislocation with the greatest care, the muscles not having then recovered tone sufficient to present any obstacle. The leg was much swelled, and a considerable effusion of blood had taken place underneath the integuments, which were detached about two inches from the bone. After the reduction of the dislocation the man was laid on a bed, and the limb placed in a straight position. There was no tendency to retraction, and therefore no difficulty in preserving its due length. Old linen wet with the saturnine lotion was then spread over the fracture. From the facility with which the bones were kept in apposition, I concluded that the fracture must be transverse. On the day after the accident the leg was still more swelled and painful; but the temperature was not increased, and there was no symptomatic fever, nor any decided marks of inflammation. Pulse 84; skin soft and natural. He had an opiate the night before, but with very little benefit; but this night I increased the dose. It procured him some refreshing sleep, and in the morning every thing seemed to be going on well. The constitution remained undisturbed, the swelling of the leg was diminished, and the effused blood seemed to be in a regular course of absorption.

From this period until the fifth day (20th) no very unfavourable symptoms, either locally or constitutionally, had taken place. But on that day the leg became much swelled, œdematous, and of a deadly pale colour; and he complained of a burning heat in the instep. Still he was young and strong, and therefore might yet do well; but on that night the most rapid and disastrous change took place. On the morning of the 21st I found the fore-part of the leg, from the knee to the instep, in a state of gangrene; and close to the fracture the integuments, cellular membrane, and superficial muscles, were completely sphacelated. The disease was advancing rapidly, and had already reached half way up the thigh, and his constitution was fast sinking under it. His stomach was irritable, his countenance sunk and alarmed in its expression, and his pulse low, frequent, and weak.

Under those circumstances the vigour of his constitution seemed to afford the only chance of saving his life, which must otherwise have inevitably terminated in the course of a few hours. I determined on giving him that chance; and, with the aid of my assistant, Mr Parry, proceeded without delay to perform the operation. I took off the limb about three inches below the *trochanter major*, which was as low down as it could be done, the integuments even there being discoloured and œdematous, showing the near approach of the gangrenous inflammation. He bore the operation with considerable patience and fortitude, but sunk extremely low after it. Three arteries required ligatures; and after they were tied the stump was dressed with sticking plaster, and lint spread with simple ointment laid over it.

After he was put to bed his stomach rejected some wine which he had taken during the operation. The skin was then covered with cold sweat; the pulse low and feeble; the eyes had a fixed vacant stare; and altogether his countenance exhibited such distress and anxiety as led me to apprehend that serious, perhaps sudden and fatal consequences, would follow the operation. Small quantities of brandy, with a few drops of laudanum, were given to him frequently, and after a few hours he recovered a little. His stomach, however, continued occasionally to reject whatever he took throughout the day; but his pulse, though low, was more distinct and firm. At night he took two grains of opium in a pill. Though it did not procure much sleep, it rendered him composed and tranquil; and in the morning he had evidently rallied considerably. His pulse was more firm and not so quick or small; the skin was soft and moist; and his countenance had assumed its natural expression. During this day his stomach became retentive, and in the evening he was altogether much improved. For several days after this the suppuration was so considerable, and the sloughing of the cellular membrane so extensive, as to require the free administration of wine, tonics, and nourishing food. I apprehended for some time that exfoliation of the point of the bone would take place; but this fortunately has not occurred; and the stump is now (3d July) perfectly healed, and the young man in the enjoyment of excellent health.

On examining the limb after amputation we found that a splinter of bone three inches long was completely detached from the tibia (longitudinally,) and forced down between the lower portion of that bone and the fibula. The fracture was transverse, as we supposed it to be.

I have been induced to send the above case for publication, as it seems to me important, from involving a question of great consequence in practical surgery; and its fortunate termination is a strong proof of the propriety of amputating without waiting for a line of *demarcation* to be formed, when the progress of mortification, as in this instance, threatened not only the loss of limb, but of life also.

Lisbon, 25th July 1827.

ART. X.—*Case of Obstruction in the Arteries from an internal Cause.* By JAMES SYME, Esq. Fellow of the Royal College of Surgeons in London and Edinburgh, and Lecturer on Surgery in Edinburgh.

ON the evening of Tuesday the 22d January I was asked to meet Mr Lizars in consultation, on the case of Alexander Young, a farm-servant on the estate of Riccarton.

I found the patient a thin, rather unhealthy-looking man, aged 58. His pulse was 90, soft, and moderately full. His tongue was moist and clean. His mind, though easily agitated, was free from delirium or confusion. His countenance expressed much anxiety and pain, which he referred to the left foot and leg.

About the middle of this leg, on its outer side, there was a large aperture in the integuments, which exposed to view the anterior tibial and peroneal muscles in a state of mortification. A bright and diffused redness bounded this part superiorly and laterally; while all the limb below on the same side, as far as the ankle, was cold and livid. There was a large slough in the centre of the sole of the foot, and almost all the toes were black either on their sides or extremities. On several parts of the instep and inner side of the foot we also observed those discoloured marks so well described by Mr Pott as precursors of the mortification which occurs in old people. The spots in question seemed to be owing to death of the cutis and separation of the cuticle, without any effusion of serum to raise it into a vesicle.

I was told that in the beginning of December he had suffered from pain in the left side, together with oppression in breathing, for which complaints he had been repeatedly blooded and blistered. He was much relieved by these measures; but on the 30th of the same month he had a relapse,—complaining of pain in the right side under the false ribs, shivering, and uneasiness in both legs, but particularly the left one. On the following day he was again blooded from the arm and leeched on the right side. A blister was then applied to the chest, and repeated sinapisms to the foot. The left leg being then painful, was subjected to warm bathing, friction with liniments, sinapisms, leeches, and, lastly, a blister, which was soon followed by sloughing of the skin over which it had been placed. About the same time a blister was applied to the sole of the foot, which also was succeeded by death of the part concerned. An incision was afterwards made through the dead skin of the leg, and a *seton* introduced. The mortification had thus existed about a fortnight previous to the time I saw him, and had not extended upwards.

Having fully considered all the circumstances now related, we were unanimously of opinion (viz. the practitioner who had treated the case from its commencement, another gentleman who had been consulted after the gangrene appeared, Mr Lizars, and myself,) that the disease, if allowed to proceed, must inevitably and speedily destroy the patient; that there was reason to think the mortification might in part be owing to the severe treatment which the limb had received in the first instance; and therefore that amputation afforded, though certainly very little, nevertheless, some chance of escape from destruction otherwise certain.

This opinion being distinctly explained to the patient and his friends, it was agreed that he should submit to the operation, which, with the assistance of Mr Lizars, I accordingly performed above the knee.

On examining the limb, which we did very carefully the following day, we found the artery, where cut across, quite pervious and apparently sound in its coats. On entering the popliteal cavity it became firmly obstructed by a dense white coagulum, like that lining old aneurisms, and continued to be so all the way to its division into the peroneal and tibial arteries, which also were obstructed to the extent of an inch and a-half from their origin; beyond this they were pervious and apparently healthy. The obstructed vessel seemed somewhat contracted, particularly about the middle of the popliteal portion, its coats were here also thicker than usual, and adhered to the clot. We could not discern the smallest speck of ossification or other morbid degeneration in any part of the artery. The vein was much thickened in its coats, so as to resemble an artery, but was not obstructed, or in other respects different from its usual condition.

As the patient bore the operation well—was freed from the distracting pain which he previously suffered—and continued to do well for several days, I flattered myself with having struck at the root of the evil. On Sunday the 25th I saw him for the first time after the operation. His appearance was then much improved, and nearly half of the stump had healed by the first intention; but his pulse and respiration were very unsatisfactory; the former being 120 and undulating, the latter also much hurried. On Monday he complained of pain in the chest. On Wednesday night he died.

The dissection was performed on Friday, in the presence of all the gentlemen who had been concerned in the case.

The remaining inferior extremity was free from any appearance of disease.

On the outer side of the stump, all the way up to the hip, the cuticle was detached, and the muscles separated from each other by a purulent effusion.

The external appearance of the lungs was quite healthy, excepting a little emphysema at the upper part, but their texture was remarkably softened, as if they had recently suffered from inflammatory action. This change was particularly observed at the posterior and lower part.

In the substance of the spleen there was a large abscess, the parietes of which were strengthened by strong adhesions to the diaphragm, stomach, &c.

The heart was extremely soft, so that the finger could be pushed through its substance with great ease. The arch of the aorta was not at all dilated, nor altered in the textures of its coats, but its inner surface was very red, and seemed as if not so smooth as usual, having a velvety sort of aspect, and appeared, moreover, somewhat softened. The abdominal portion of the aorta was filled with dark half-coagulated blood, but retained its usual colour. The right internal iliac artery was firmly obstructed from its origin, as were the right and left internal iliac veins. The left femoral artery was pervious all the way to its extremity, where the ligature remained, but the profunda and corresponding vein were firmly obstructed, as were also the same vessels of the right side. The right femoral was pervious, but the popliteal was obstructed precisely as I have already described the left one, which was amputated.

We searched carefully for traces of chronic disease in the arterial system, but could find none; not the smallest trace of osseous or other degeneration. I have since found a small scale at the origin of each internal iliac, but how inconsiderable, and how little affecting the inner surface of the artery, will appear from the fact of its having been overlooked at the dissection, when some of the parties present were very diligent in searching for such morbid changes.

I do not recollect of ever dissecting a subject so far advanced in life whose arteries were in a more healthy condition as to texture.

My opinion of the case is, that it was one of acute inflammation of the arteries, similar to the one recorded in Mr Hodgson's Treatise on the Blood-Vessels, and similar to those very rare cases in which the vessels were obstructed from effusion consequent on inflammation of their coats.

The different obstructions found at the dissection must either have existed at the time of the operation or taken place after it. If they did not exist then, there was nothing to prevent the operation from being successful, and if they existed when it was performed, then it must be allowed that the collateral circulation which had sufficed so long might have nourished the limb permanently; so that, in either view of the case, the operation afforded a chance of recovery.

As to the general question respecting amputation on account of gangrene, I certainly think that the operation is not proper when the disease commences without any external cause. Thus a few days before the case above related happened, I was consulted respecting a patient about 40, whose foot and leg had mortified, without any apparent cause whatever; but though the friends were very anxious to have the limb removed, I would not consent to do so, as there could be no doubt that the disease would continue to extend after the operation.

In conclusion, I venture to suggest that obstruction of arteries, as a consequence of effusion from their own coats, may not be so rare an occurrence as it would appear to be, from the extreme rarity of such cases on record. It has often been remarked that the vessels leading to or from a mortified part were rendered impervious by coagulum; and this circumstance has hitherto been explained loosely as a consequence of the mortification, as an effort of nature to prevent hemorrhage. But it is well known, as I can testify from repeated observation, that the vessels are not always obstructed, which they ought to be if the obstruction were the effect of mortification. And, therefore, I think it may reasonably be asked, is not the obstruction rather the cause of mortification in those cases where it exists? The coagulum certainly does not look like one resulting from more stagnation of blood in the vessels; since, instead of being soft, bloody, and *smaller* than the calibre of the vessel, it is firm, white, and closely impacted, so as to render it difficult to lay open the vessel without injury to its contents.

I have long been satisfied that ossification alone is not sufficient to occasion mortification, since, were it so, seven people out of ten who have passed the age of sixty ought to suffer from mortification of the toes; and I have no doubt that, in addition to the ossification, there is some farther obstruction, as from contraction of the vessel, of which there is a specimen in my possession, the aorta hardly admitting the little finger, or from coagulation, of which also I have a specimen, in the femoral artery of an old man who died of this miserable disease. The vessel is completely plugged for the extent of five or six inches, by an exceedingly firm white fibrinous coagulum.

75, George Street,
20th February 1828.

Art. XI.—*Case of Tubercular Tumour of the Brain terminating in Hydrocephalous Effusion.* By Robert Evans, M. D. M. R. C. Surgeons, London.

May 22, 1826. J. N., aged 11 years, was a young gentleman of great acquirements and classical knowledge for his age, the son of a learned clergyman. He was reported to have had a discharge of blood from the bowels, with a protrusion of the rectum, till he was eight years old; and for the last six years this discharge took place occasionally while at stool. This ceased latterly for several months; and since that period he complained of a severe pain of the head, at times more acute, with slight giddiness, muscular debility and languor, listlessness, and disinclination to make his usual exertion. The bowels were costive, with deficient secretion of bile. The head was naturally very large.

He was ordered alterative doses of blue pill, with aperient medicine; the head shaved, and an evaporating lotion applied; leeches to the anus, and if not relieved by them, to the temples also, and to abstain from animal food.

By this treatment for some time he was much benefited, and took horse exercise. This was a truce of short duration only; for in a little time (18th June 1826) he complained of more decided symptoms of cerebral disease, as severe headach, great throbbing of the cerebral and carotid arteries, giddiness, double vision, irritability of stomach, and at times full vomiting; stupor, lassitude, and considerable dilatation of the pupils; and a sense of heat in the head, though without much change of colour in the face. The appetite was bad; pulse 60, and small; no thirst; no heat of skin, though restless; urine a little cloudy; bowels costive; and feet cold. He expresses the feeling in his head to be a throbbing, bursting sensation, forcing open the skull. A brother of this patient died some years ago with similar symptoms, and at the same age; but no dissection took place.

18th June 1826.—For the above symptoms the head was shaved and blistered; and, though the pulse did not indicate it, xviij ounces of blood were drawn from the arm; ten leeches were applied to the temples, and cold to the head. The nape of neck was blistered, and the surface was to be kept discharging with savin cerate. Brisk purgatives were exhibited, with submuriate of mercury every fourth hour till the mouth became affected, combined with squill and digitalis in fit doses, and low diet. The feet were immersed in warm water. After venesection the pulse rose twenty beats, with perfect relief to the head and stomach.

22d June.—Complains very much of the head; vision double; pulse 88; dejections semifluid, dark-coloured, and glossy on the surface; head to be well and often rubbed with tartar emetic ointment; venesection to ℥xvj., when he fainted; calomel and James's powder each two grains twice daily; diuretics continued.

26th June.—Much better, and medicines continued.

28th June.—Is better; pulse 70, irregular; dejections yellow; head easier; vision was better, but to-day is again double. No effect from the diuretics.

3d July.—A sense of fulness in the head; double vision, and a mist before the eyes obscuring objects; immense flow of urine; mouth very little affected; pulse 80. Twelve grains of strong mercurial ointment were ordered to be rubbed into the arm-pits night and morning; calomel and James's powder to be continued; weak chicken-broth and bread, which agree with his stomach.

10th July.—Was better, but to-day is very ill, nearly insensible. The head appears much enlarged, and the sutures feel as if separated. The mercurial fœtor is great, and the gums are red, but not sore; the pupils greatly dilated; he voided his urine once insensibly; requires aperient medicine; pulse 60.

14th July.—The two days previous he made still a larger quantity of urine, is more sensible, and felt no pain since the 10th. The head seems smaller, and the sutures are not felt so sensibly separated. Sleeps quietly and sound; has more appetite and desire for drink; complains of cramp in legs and hands, with numbness and sleepiness, as he calls it, particularly in the right, but which are sometimes relieved by friction. He is almost blind, pupils dilated, and iris like a narrow ring. The mercurial ointment, calomel, and James's powder continued; to have beef infusion, chicken-broth, albumen of egg beat up with milk and water, and ripe fruit.

20th July.—Is free from pain; makes much water; no return of spasm; sleeps well at night; mouth not much affected. Continue medicines. A seton inserted in neck.

28th July.—Continues free from pain; is much affected by the mercury; no alteration in sight. As the action of the heart is much affected, the digitalis is omitted. Is cheerful, and joins in conversation; takes nourishment. Is ordered a mild bitter as a tonic to stomach; frictions with stimulating liniment to spine, legs, and thighs; bowels regulated by colocynth pills. The reported protrusion of rectum is now found to be a highly vascular fleshy tumour, with a long flat neck, which was removed by ligature.

This degree of convalescence continued to the 7th August, when he experienced a renewal of all his former sufferings; pain in the temples and forehead, chiefly in the evening; giddiness, and an almost constant involuntary rolling of the eyes; pulse 108 to 110. Mercury was again used, and antimonial ointment to the head; diuretics, &c. as before.

In the present attack the pain was so violent as to induce severe and general convulsion and apparent apoplexy, ending in profound sleep, and returning with short intervals of ease; also most excruciating pain in the loins, groins, and front of thighs, with vomiting of acid matters. His distress was so great that he prayed to be relieved from his bodily sufferings. He is emaciated to a skeleton; blind, and unable to make the slightest exertion.

From this state he was again relieved, and continued more sensible, with good appetite, regular bowels, and sound sleep, and without pain or convulsion; but the eyes were constantly rolling and turned downwards, and a feeling as if his body was constantly sliding down from his chair.

29th September.—No permanent benefit having been obtained from the treatment pursued, and various other remedies not mentioned, and pain returning at intervals, frequent convulsions, and tremors of the legs, with inability to raise or move them, though possessing sensation, he was merely attended to with nourishing diet, medicine, and enemata to regulate the bowels. Though at intervals free from pain, at others it was most excruciating, with frequent convulsions. He lingered till 26th June 1827, when death released him from his sufferings. He was sensible to questions, and his mental faculties were clear almost to the last.

He was scarce able to raise his hands to his head; and the feces and urine were passed unconsciously for several weeks previous to death. When near dissolution, and scarce able to speak from weakness, and when addressed by his parent on the subject of eternity, his intellectual functions were shown to be unclouded to the last moment by the clearness of his replies. A medical student, a relation, was the greater part of his illness constantly with him.

Dissection twenty-four hours after death.—A clergyman and the medical gentleman mentioned were present. The body was so emaciated as to resemble a skeleton covered with skin.

Head.—On the upper and anterior part of right frontal bone was a tumour about the size of a hazel-nut, soft and compressible, but not so large as during life. When the scull-cap was removed, we found in it seven perforations two and half lines wide, and several smaller ones, some on the upper part of the frontal bones near the coronal suture, and in the line of the saggital on the parietal bones. These perforations were not from caries, but absorption from internal pressure, and were merely closed by a fine delicate membrane, not stronger than the arachnoid seen in the base of the skull, and covered by the common integuments. The whole internal surface of the scull, particularly in the direction of the sinuses, was rough, and studded with numerous spiculæ, not from osseous deposition, but remaining from bony absorption; and generally the substance of the skull very thin and nearly transparent, except a patch at the upper portion of the parietal bones, which was morbidly thick and dark-coloured. The dura mater was perforated by the tumours, which corresponded to the openings in the skull, and its vessels tinged with dark blood, though the sinuses were empty when slit open; and it adhered firmly for a small space to the upper portion of the right anterior lobe of the brain, and in other respects healthy. The falx was healthy, but adhering also firmly to that part of brain similar to dura mater. The pia mater loaded with blood, and easily removed from among the convolutions; and its removal gave vent to a small quantity of limpid fluid.

The convolutions and intergyral spaces seemed in a sound state, and in usual proportion all over the surface, except on the right anterior lobe, which was a mass of apparently disorganized substance, with a surface unequally projecting, like a mass of tubercles agglutinated together, and hard to the feel. On exposing what is termed the *centrum ovale*, the interior of the brain resembled a bag of fluid buoying up the *corpus callosum*, and distending the surrounding substance. The left lateral ventricle contained a quantity of fluid clear as rock water. The *plexus choroides* was small, and of a pale colour.

The right ventricle was enormously distended with a brownish-coloured fluid, that coagulated on being exposed to the air. The floor and side of this ventricle next the *corpus callosum* were lined by a layer of apparently organized lymph, closely adhering to the subjacent surface, about a line thick, highly vascular, and smooth surface, and when cut into resembled the cortical substance of the brain. The *plexus choroides* was larger. The outer wall of the right ventricle, and indeed the whole of the anterior lobe, was a mass of tubercles, of various hues, and from the size of a hazel-nut to a hen's egg, of a light dove-gray and yellow colour, highly vascular, except the centre of the general mass, which was friable, and of a whitish yellow colour, like a custard. Some of the exterior part cut like semicartilage. Some of these tubercles lying in the ventricle were easily separated from the general mass, and were friable in the centre ; and in one there was a small elliptical cavity lined with a fine glossy membrane. There was not a vestige of medullary substance in the right anterior lobe nor pia mater. There were some small tubercles on the outer surface of the left anterior lobe, but not dipping beneath the cortical substance, corresponding to the absorbed portions of the skull, of a pulpy consistence and whitish yellow colour. These perforated the dura mater. It was thought there could not be less than sixteen ounces of fluid in the head. It could not be collected.

The Cerebellum was healthy. Limpid fluid was effused between the dura mater and upper portion of *medulla spinalis*, but its quantity could not be ascertained.

Abdomen.—Intestines distended with air ; hard feces in rectum and colon ; stomach empty and small ; veins of omentum filled with dark blood ; gall-bladder distended with dark-green bile ; liver and other viscera healthy.

Thorax.—When opened, the lungs were found not nearly to fill the chest, but crepitous all over their substance. No flnid was contained in the pleura or pericardium, which was in other respects sound. Heart generally large ; right auricle very large ; and both auricle and ventricle contained black fluid blood ; the walls of the latter soft, flabby, and two lines in thickness ; its capacity large, and was judged four times larger than the left ; valves very thin. Left auricle before cut into was about the size of a goose egg, and hard ; its capacity very small, and its walls of the base six lines in thickness. The margin of the valves at the aortic aperture, for a line in breadth, was very closely studded with morbid growths, semicartilaginous, from the smallest point to a pin's head. *Carneœ columnœ* very large, and nearly filling the cavity of the ventricle ; left auricle very small and shivered ; coronary veins filled with blood; large vessels healthy. There was a growth of a solid bone, ending in a point, near three inches in length, growing from the internal condyle of the *os femoris*, point standing upwards.

Magherafelt, Ireland,
1st November 1827.

ART. XII.—*Case of Hypertrophy of the Heart, with Cartilaginous Induration of the Mitral Valve.* By HENRY VERNON, M. D. M. R. C. S. L. Extraordinary Member of the Royal Physical Society Edinburgh.

A YOUNG lady, 31 years of age, of a spare delicate figure, florid complexion, interesting appearance, and of an extremely cheerful disposition, which led her acquaintance generally to suppose she was strong and robust, was reported by her mother and her immediate connections to have experienced a gradual but striking change in the state of her health since she arrived at the age of 17. Soon after that period, though still retaining her natural buoyancy of spirits, her bodily strength, which previously had been perfect, appeared evidently to be unequal to them. She lost much of her *embonpoint*; but it was particularly noticed that she became painfully distressed in her breathing on every slight exertion, and was obliged to desist immediately from persevering in any thing that had occasioned it, and to have recourse to the horizontal position for relief. She was free from any habitual cough, only experiencing fits of it when respiration had been thus accidentally disturbed. This report of symptoms I was enabled to collect from herself and friends prior to my being consulted, which was about six years and a half before her death, and during which period I was in the habit of seeing her very frequently. Soon after the first symptoms of impaired health were disclosed she applied to a medical gentleman, who conceived at that time that there was an affection of the spine. The portion of the column where the disease was supposed to exist was treated accordingly, without much relief.

From the absence of cough, and all symptoms of any idiopathic pulmonic affection, I was induced to inquire into the state of the heart. I found its action to be of a most tumultuous and confused character. Its impulse against the ribs was excessively strong, with an area of moderate extent, and imparted a grating sensation to the hand. The pulse at the wrist was feeble in the extreme, and very irregular; it gave a comparatively firm beat at an interval of about every twelve seconds, and the intermediate time was occupied with a confused undulatory motion; and though I was in the habit of examining her pulse frequently, I never found this condition to vary.

As this state of things had continued stationary for a length of time, and her usual habits had suffered but little interruption from it, I made no very minute inquiry into the condition of the organs contained in the chest. She seldom indeed applied for relief, excepting when her breathing became more than usually distressed from mild pulmonary catarrh, to which she was subject during winter. During the last two years preceding her death, the palpitations and difficulty of breathing increased in violence, and obliged her to keep the house, and even to recline in the horizontal position a great portion of her time. Simple catarrhs, which in most persons would have been of the most trifling character, produced in her case severe asthmatic paroxysms. During this latter period of the complaint, when applied to for relief under these severe asthmatic attacks, I employed the stethoscope to ascertain more minutely the condition of the thoracic organs.

The chest sounded well on percussion, excepting over the left præcordial region, where the sound was dull and obscure. The impulse of the heart between the fifth and sixth ribs was excessively strong; the shock repelled the hand with much force; and its area was of moderate extent. The sound of the ventricular contraction was very dull and prolonged, and masked that of the auricle. That produced by the contraction of the right cavities was much clearer, and could be heard over a greater extent. The shock imparted to the hand applied under the sternum was tolerably firm.

The respiratory murmur was distinct over every part of the chest, excepting over the præcordial region, where it was often with difficulty detected. When the palpitations and difficulty of breathing were more than usually aggravated, the murmur was intensely puerile. At the commencement of those violent paroxysms, which were occasioned by mild pulmonary catarrh, I generally found a slight degree of the sonorous rale, which soon vanished, a copious and free expectoration being always speedily established after the application of leeches and a vesicatory to the chest. This expectoration always relieved the aggravated symptoms, and in the course of five or six days from the first attack she was usually restored to her former state. In the last fatal attack the mild catarrhal symptoms had yielded as beforetime; but the palpitations had increased much in violence, and the saw-creak noise described by Laennec was heard very distinctly with the stethoscope. This state of things continued for two days and a night, during which time she may be truly said to have been dying in lingering agonies, when her sufferings were at length closed.

Permission was given for a necroscopic examination, which I made, assisted by my friend Mr Parks, surgeon of this place.

Dissection.—There was great emaciation, with turgescence of the subcutaneous veins of the face, neck, and trunk. Chest resounded well. On raising the sternum the pleura was found in a natural state. The lungs collapsed on the admission of air, and were in an extremely sound state, being crepitous throughout. They were almost concealed on the left side by the amazing size of the heart; and there was a very slight adhesion of a small portion of the left lobe to the *pleura costalis.* The chest contained no serum. The pericardium contained about three ounces of a reddish fluid.

The heart was three times the size of the closed hand of the subject. The right auricle and ventricle were enormously distended with blood; and from them were drawn large pieces of fibrin, as thick as the fore finger, and of the colour and consistence of yellow fat. They were deposited in greatest abundance beneath the tricuspid valve, and at the mouth of the pulmonary artery. When the right cavities were emptied of their blood, they collapsed; but the heart still preserved its enormous dimensions. The cavities of the auricle and ventricle were exceedingly dilated; and the ventricle was at the same time more than double its natural thickness. The tricuspid valve was perfect. The left auricle offered nothing remarkable; but the left ventricle was in a state of excessive hypertrophy, the thickening extending internally, so as to narrow the dimensions of its cavity in an extraordinary degree. The mitral valve was in a highly defective state from disease. Its base was converted into a thick fibro-cartilaginous substance. The valve itself was studded with ossified points, and gave a sensation as if hard gritty fragments were pressed between the fingers. The edge of the valve was also affected; and its pointed processes were connected by a thick cartilaginous cord, which, instead of being applied flat and close to the sides of the ventricle when the auricle contracted, stood up in some measure and formed a circular orifice, which would barely admit the end of the fore finger. The *columnæ carneæ* were very thick and strong. The *chordæ tendineæ* had also become cartilaginous and very much contracted, so as to prevent the valve from closing during the contraction of the ventricle. The parietes of the ventricle were triple their natural thickness.

Considering the gradual progress of this disease of the heart, which cannot be supposed to have undergone any sudden increase within a short period of the patient's death, though I can easily conceive that sudden and momentary death in such a state of the organ might have occurred at any time under any extraordinary exertion, I cannot satisfactorily account for the patient's sinking under a train of increasingly fatal symptoms of several days' duration, unless that their immediate cause was the formation of fibrinous concretions. The disease would evidently occasion an accumulation and stagnation of the blood in the right cavities of the heart, thus favouring the spontaneous separation of its constituent principles. In the case I have been reporting, examination disclosed nothing in addition to what we may presume had been the condition of the heart for the last three or four months at least. The lungs were sound and crepitous throughout; the catarrhal affection had left no vestige of its existence, with the exception of a small portion of the bronchial membrane in the large ramifications, exhibiting here and there a small patch dotted with red points.

Under these circumstances, it is difficult to account for this gradually increasing train of fatal symptoms. I am therefore led to believe, that, when this and similar diseases of the heart have arrived at a certain degree, the termination of life is rapidly accelerated by the deposition of fibrinous concretions, and that they are the immediate cause of the last closing scene.

Bury, Lancashire, 14*th November.*

ART. XIII.—*Case of Active Hypertrophy of the Heart.* By JAMES BLACK, M.D. of the Royal College of Physicians, London.

W. H., a porter at the coach-offices, aged 36, stout, short-set, and of a muscular frame, complained for the first time to me, on the 1st June 1826, of great oppression, and a dull pain in the left side of his chest, of much dyspnœa, and a sense of suffocation in his bed at night; of weakness, and of a violent throbbing pain occasionally in his head. The countenance was anxious, generally pale, changing at times to a livid hue. Thirst, appetite bad; but no cough nor expectoration. The pulse was hard, quick, and irregular; and the throbbing in the course of the carotid was sufficiently strong to shake his head. The beating felt in the region of the heart was dull, but of wide extent and impulse. No stethoscopical examination made. Urine scanty, and there appeared to be a tendency to anasarca and abdominal dropsy.

Diuretics of digitalis, squill, and a little colchicum, with blistering and anodynes, were administered, and perfect quietude enjoined. But after a month's increasing suffering, with corresponding exhaustion, he was suddenly carried off on coming to the erect position.

Leave being given to inspect the body, with the assistance of Mr Platt and Mr Wilkins, surgeons of this town, the left lung was found natural and crepitous, and there was no pleural effusion. The right was natural in its parenchyma, but it was surrounded with numerous fibrinous exudations, which attached it to the costæ, and formed large cells filled with nearly a pint of serum. The heart presented the most interesting appearance, being double its natural size, and its left hemisphere was of a livid vascular hue. The walls of the left ventricle were about one inch in thickness, and extremely vascular, resembling, when cut, a section of the recently gravid uterus. The right and left cavities, as well as the aorta, were filled with much dense coagulated blood.

The more particular morbid lesion or disorganization was the state of the aortic valves; one only being perfect, a second loose, acutely triangular, and useless in function; while the third was only a small tubercle. Behind the loose and imperfect valve was the orifice of the coronary artery, which was as large as a goose-quill. The root of the aorta was much thickened. Its inner coat was of a florid red colour, and studded over with red *carunculæ* and capillary vegetations. The mitral valves were imperfect, but uninflamed; and the sigmoid ones of the right side were of perfect formation. The right ventricle was not nearly so large as the left. Its parietes were denser, paler, and about one-fourth to one-half the thickness of those of the left. The tricuspid valves and the auricle were natural; but the lower part of the fœtal *foramen ovale* was open, to the size of a goose-quill, into the left auricle; and the mouth of the coronary vein was wide enough to admit the point of the little finger.

In this case the primary lesion seemed to have been in the valves of the aorta. Perhaps it was partly congenital; and the continued exertions of the patient's life had the effect of rendering these valves more imperfect. The feebly-supported column of blood would consequently, in the progress of time, react on the heart itself, excite an inordinate action in the organ from the increasing effort required to propel the blood; and its bulk and vascularity would be accordingly increased, the coronary artery being always open to the full impulse of the ventricular current. Valvular inflammation would also likely ensue, as well as an impeded circulation through the lungs. The violent throbbing of the carotid evidently arose from the hypertrophical action of the heart driving the blood into vessels relatively reduced in their muscular tension; as on examination these arteries were found to be of a natural size, though during life their extended pulsation denoted their condition to be more or less aneurismal.

Bolton, May 1827.

ART. XIV.—*Case of Organic Disease, chiefly in the Ovaries.*

By ALEXANDER MURRAY, A. M., Licentiate of the Royal College of Surgeons, Edinburgh.

THE diseased parts which I intend to describe were met with in the Hospital de la Pitié at Paris, in the end of November last, in the body of a woman apparently about 50 years of age, that I dissected, in conjunction with Mr Henry Grant of this city. The appearances seemed to me of an interesting character, and I therefore examined them with some attention, and took notes of what I saw. As I have since looked through the works of Bonetus, Morgagni, and Baillie, where I have met with nothing exactly similar, and considering also that this case shows the manner in which the viscera of the abdomen and pelvis are probably in general arranged in ovarian dropsy of a large size, an arrangement known no doubt to those who have had it in their power to examine the disease by dissection, but not to be satisfactorily gathered from any work with which I am acquainted, I have been induced to offer for publication an account of the circumstances.

The belly was as large as that of a female far advanced in pregnancy; but in place of the firmness and uniformity of pregnancy, it had an unequal, doughy, slightly elastic surface, and was not more prominent in front than at the sides. Upon opening the abdomen fully, nothing at first was noticed but three bluish tumours; one with a few vesicles on its surface, projecting from under the right ribs, somewhat in the manner of an enlarged liver; another like a greatly distended urinary bladder, rising up out of the pelvis; and the third, smaller and more unequal on its surface than the others, lying upon the right *venter ilii.* The first tumour proved to be the right ovary, or a diseased substance in its room; the second was an appendage to the first, passing down to the very bottom of the pelvis, and filling that cavity so completely that, as happened to Morgagni in a similar case, a noise was heard in removing it, like that produced by drawing an exhausted cupping-glass from the skin; and the third tumour was the left ovary. The first when opened was found to consist of several sacs, of which one was larger than all the rest, unconnected with one another, and filled with a muddy fluid; while the second, of which the covering was whiter than that of the rest, was found to be a single cavity, and to contain a fluid exactly like pure water. The third tumour consisted of two cells, and contained an inky fluid. It may be added, that all the tumours were smooth both outside and inside, except a portion of the inner surface of the first tumour, where various clusters of small hard white eminences were observed. Upon looking narrowly, but without deranging the original view, small portions of the transverse and the descending colon were seen, and the uterus was discovered below the left ovary, and over the ascending ramus of the right *os pubis.*

When the tumours were removed, the pelvis, and the abdomen below the level of the lower extremity of the sternum, exhibited an almost entirely empty appearance. The small intestines were pushed up behind the colon, where they occupied an incredibly small space. The large intestines presented nothing remarkable, unless that the rectum and the lower part of the colon were pressed quite flat, so as to resemble a piece of riband. The stomach was empty, and a small fat-like pear-shaped tumour was attached to its inner surface, near the pylorus. The omentum did not project beyond the colon. The liver was of healthy structure, but not more than half its usual size, and it was in a manner folded or turned round, so that the small lobe became anterior to the other; and the division between the lobes, in place of lying over the spine as is usual, lay close upon the right ribs. The right kidney was found between the right lobe of the liver and the back-bone, with its lower extremity nearly three inches above the superior-posterior spinous process of the ilium. From this kidney being higher than is usual, the left became the lower of the two, and on its surface was a small cavity containing cheesy matter. I have no memorandum of the state of the pancreas and spleen. The ureters were both enlarged, especially the left, which in some parts was more than twice as thick as a common goose-quill. From the situation of the left ovary, its spermatic artery was necessarily several inches longer than is usual. The situation of the uterus was already mentioned. That organ was at first supposed to be much diseased, as it had a stony hardness, but this was found to arise from its containing a calculus or a bony concretion; viz. a hard yellowish-white mass, covered at one end with a fleshy crust, loosely connected to the uterus, and unequal on its surface, like the fruit of the genus *Rubus.* Indeed, at the time, I was much struck with its resemblance in shape, size, and somewhat in colour to the cloud-berry or aveiran, the fruit of the *Rubus chamæmorus.* The uterus was drawn out to the length of five inches, but the vagina was not supposed to be altered. The lower part of the uterus, as well as the bladder, was compressed against the pubes, and it was found impossible to distend the last-mentioned viscus to a size beyond that of a turkey's egg. The arch of the aorta was dilated, and the semilunar valves of the left side were hardened and thickened. No other diseased appearance was observed, unless a degree of œdema of the inferior extremities. The general condition of the body was not that of one who had been long suffering from a formidable malady.

There is a difficulty at first sight, which will strike any one in explaining why the left ovary should be found quite in the right side; especially when, in consequence of the situation of the tumours marked 1 and 2 in the subjoined sketch, a complete barrier was formed between the two sides of the body; and this difficulty is increased by the consideration, that, in ordinary cases, though then no such barrier as the above exists, the parts have not, as far as I can learn, been ever found altered in the present manner. Indeed, the fact, that the ovary, after enlarging to a certain size, quits usually its situation in the pelvis, and rises up into the abdomen, is to me not altogether void of difficulty; though more than one of those with whom I have conversed think it of easy explanation. Be this as it may, there is no doubt that such a change of situation does in general occur; and that consideration will, I think, enable us to account for all the appearances before us. The right ovary, after undergoing a certain augmentation, would become situate on the right *venter ilii,* and, by making this change of place, its peritoneal connection with the uterus would first be put on the stretch, and afterwards the last named organ would, through being elongated, follow the ovary. In a similar way would the other ovary be dragged after the uterus; but it is to be noticed that the uterus would not move to the same extent as the right ovary, nor the left ovary to the same extent as the uterus; for of the movement made by the right ovary, part would be lost in putting on the stretch the connection between itself and the uterus; and then of the movement made by the uterus, part would be expended in putting on the stretch its connection with the left ovary. In short, the change of place on the part of the left ovary would be much less than that of the right. The left ovary, however, being thus *so far* displaced, I suppose that then commenced the formation of the large bladder attached to the right ovary. As this increased it would pass down into the pelvis, and upon coming to the level of the left ovary, which had been already drawn a little to the right, it would of course pass down through the space vacated by the ovary,—that is to say, on the left of the place which the ovary had now come to occupy. Having got to this situation it would, in the course of its increase, clearly contribute to push the ovary in question farther up, and farther to the left than otherwise it would have gone.

I regret that the uterine concretion could not be submitted to chemical examination, as it was lost. Uterine calculi are so rare that Baillie says he never met with one; but I am disposed to believe that I saw a case of that kind a few years ago at Alford, in Aberdeenshire. A woman of sixty had occasional discharges of blood, not occurring when the urine was passed, but at other times; and among the blood was frequently found a quantity of calcareous-like matter, which was usually loose, but on one occasion concreted into a lump, not, I think, less than a hazel-nut. Though I do not clearly know that the distinction which Dr Baillie draws between a calculus and a bony concretion can be in all instances observed, I may remark that the substance already mentioned, as having been found in the uterus of the person who forms the subject of this communication, was probably of the kind that this author had in view, when he speaks of " a bony mass in the cavity of the uterus" as follows :—" In the cavity of the uterus a bony mass is sometimes found. When this is the case, I suspect that the hard fleshy tubercle within the cavity of the uterus, such as we lately described, had been converted into bone. This at least had taken place in the only instance that I have known of it; for a great part of the tubercle had still remained unchanged."

The tumour in the stomach cannot be considered as necessarily connected with the other circumstances, but it must have greatly impeded the transmission and digestion of the food; and it is a curious occurrence, considered as having taken place in a case where, from other causes, viz. the compressed state of the lower part of the bowels, &c. the passage of the alimentary matters was not a little obstructed. The enlarged state of the ureters no doubt arose from the difficulty with which the urine found its way from the kidneys. The state of the bladder is not unworthy of notice. This, from the pressure of what I may call the ovarian bladder, had not, perhaps for a long time, contained more than a very few ounces; and when the pressure was removed, the organ, though sound, could not be distended beyond that size to which, for some time past, it had been accustomed. This fact shows that, in certain diseases in which the urine is frequently voided, as in stone, the contraction of that organ observed after death may arise, not from the accompanying derangement of structure, but merely from the cavity having become adapted to the quantity of liquid which for some time past it had contained; in other words, that it is the effect and not the cause of the defective power of retention.

In describing the parts, I deferred noticing the connection between the uterus and ovaries. When I met with this case I was not well acquainted with the appearance assumed by the Fallopian tubes in cases like the present; and in my notes I have stated those tubes to be the connecting *media*, by adhering to the ovaries; but a gentleman versed in matters of this nature, to whom I showed the sketch of the parts, has assured me that those connections are here merely peritoneal bands, containing branches of the spermatic arteries. I still, however, think it probable that the Fallopian tubes, which, in this disease, as they lose their characteristic fimbriated margin, are readily overlooked, were adhering to, and confounded with the peritoneal productions. In relation with the attachment of the ovaries, and with the subject in general, there is a question which it would not be out of place shortly to consider, namely, the propriety of extirpating the ovaries when dropsical; but I must first be allowed to devote a few words to the other modes of treatment that have been resorted to.

With respect to internal medicines, I can find no reason for believing, that in a single instance they have proved of the least service.

Tapping the ovary is an operation that, for various reasons, is, I fear, entitled to slender praise. In the first place, when övarian dropsy is not examined until it has made a considerable progress, it is far from easy to determine whether it exists. In the early stages, indeed, the diagnosis is less difficult; for at that period, especially if the person is thin, the enlarged ovary may often be felt, so as to leave little doubts of its nature, as was the case, for example, in a patient of my own, in whom, after certain hysterical symptoms, a tumour, unquestionably the ovary, made its appearance in the *venter ilii*, where it could be grasped with the utmost facility. But we are not usually consulted on this affection until it has made considerable progress, and then the diagnosis becomes much more obscure. A more signal illustration of this difficulty could scarcely be conceived than one of Mr Lizars's cases, in which, though numerous medical men of eminence, as it is stated, were unanimous in pronouncing the ovaries to be diseased, it turned out, that, unless fatness and a slightly curved spine, there was nothing whatsoever to account for that mistaken opinion. Farther, should we be certain that the ovary is the diseased organ, we are still ignorant whether it contains any thing that can be evacuated by the canula. The organ may be converted into a soft fleshy mass, or it may contain an elastic gelatinous matter; and those who have attended the most to such subjects are the most diffident about ascertaining with precision the existence of a liquid by the touch. In fact, any thing soft and elastic communicates to the hand a sensation like that produced by a liquid. Thus, in one of the cases related by Mr Lizars, it appears to have been found impossible to distinguish the nature of the ovarian tumour, even when the operator was in close contact with it. He plunged his instrument into it several times, proving that he believed it to contain a liquid, but nothing followed the perforations. Various other instances where puncture of the ovary gave exit to nothing are upon record. Again, another difficulty presents itself in the circumstance, that in ovarian dropsy we can hardly ever by tapping give more than a partial relief, as the fluid is almost invariably contained in several cells. Lastly, Morgagni assures us that the bowels have been found before the enlarged ovary, and thus in the way of the trocar.

After drawing off the water, it has been proposed, and sometimes practised, to inject a stimulating liquid, for the purpose of exciting inflammation and consequent adhesion in the inner surface of the sac. Now, though in the ordinary textures of the body inflammation is often followed by the adhesive process, it by no means follows that a surface like that of the ovarian sac, which some indeed consider a substance altogether new, shall exhibit the same phenomenon. The doubt suggested by reasoning is strengthened by experience; for though fatal inflammation has in various instances taken place from injecting the ovary, I am not aware that there is a single well authenticated instance of the cavity of the sac having become thus obliterated.

Against the extirpation of the ovaries when enlarged from dropsy or any other cause, we have the difficulty of determining with certainty that the ovary is the seat of disease; and did we know this viscus to be the part affected, we cannot tell whether it does not so adhere to the neighbouring parts as to prevent its removal. In other words, we must perform an operation, unquestionably of a highly dangerous character, before we have the least knowledge that the object for which we do operate, namely, the removal of the tumour, can be attained. In the case which gave rise to this paper, it would indeed have been easy to remove the tumours, as they were attached only through the peritoneal bands already noticed; but how was this to have been foreseen? The cases published by Mr Lizars have been pronounced by good authority to have a value, as they show that operations on the abdomen may be at-

Mr. MURRAY'S CASE

Fig. 1.

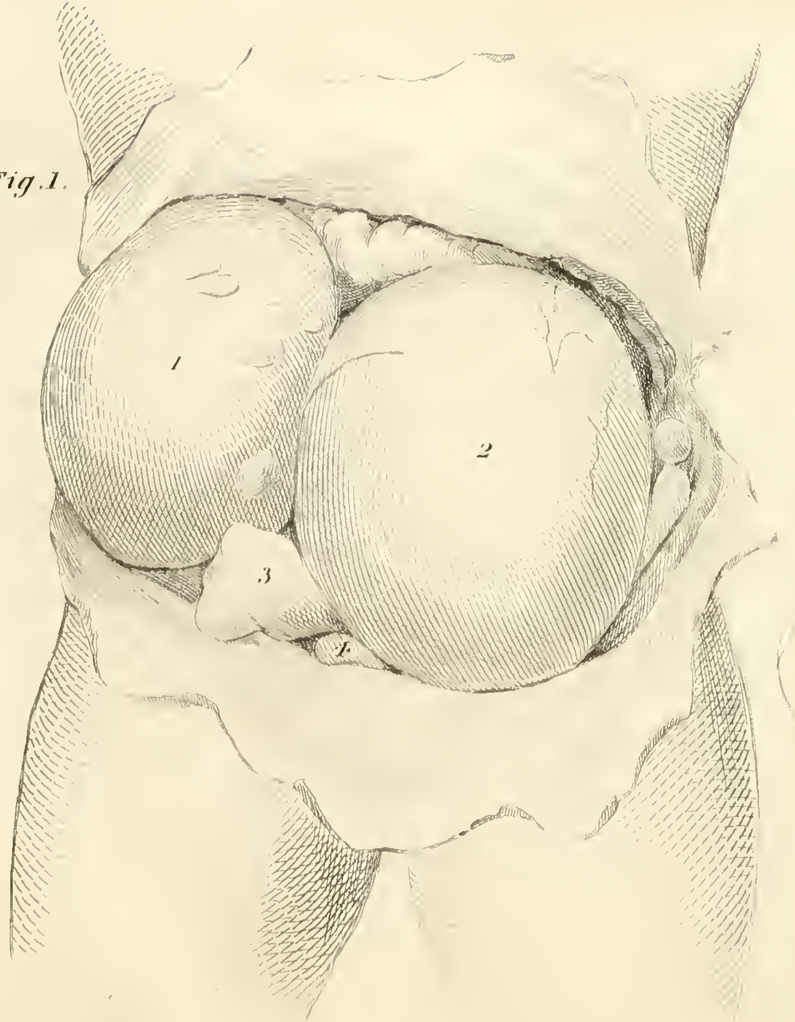
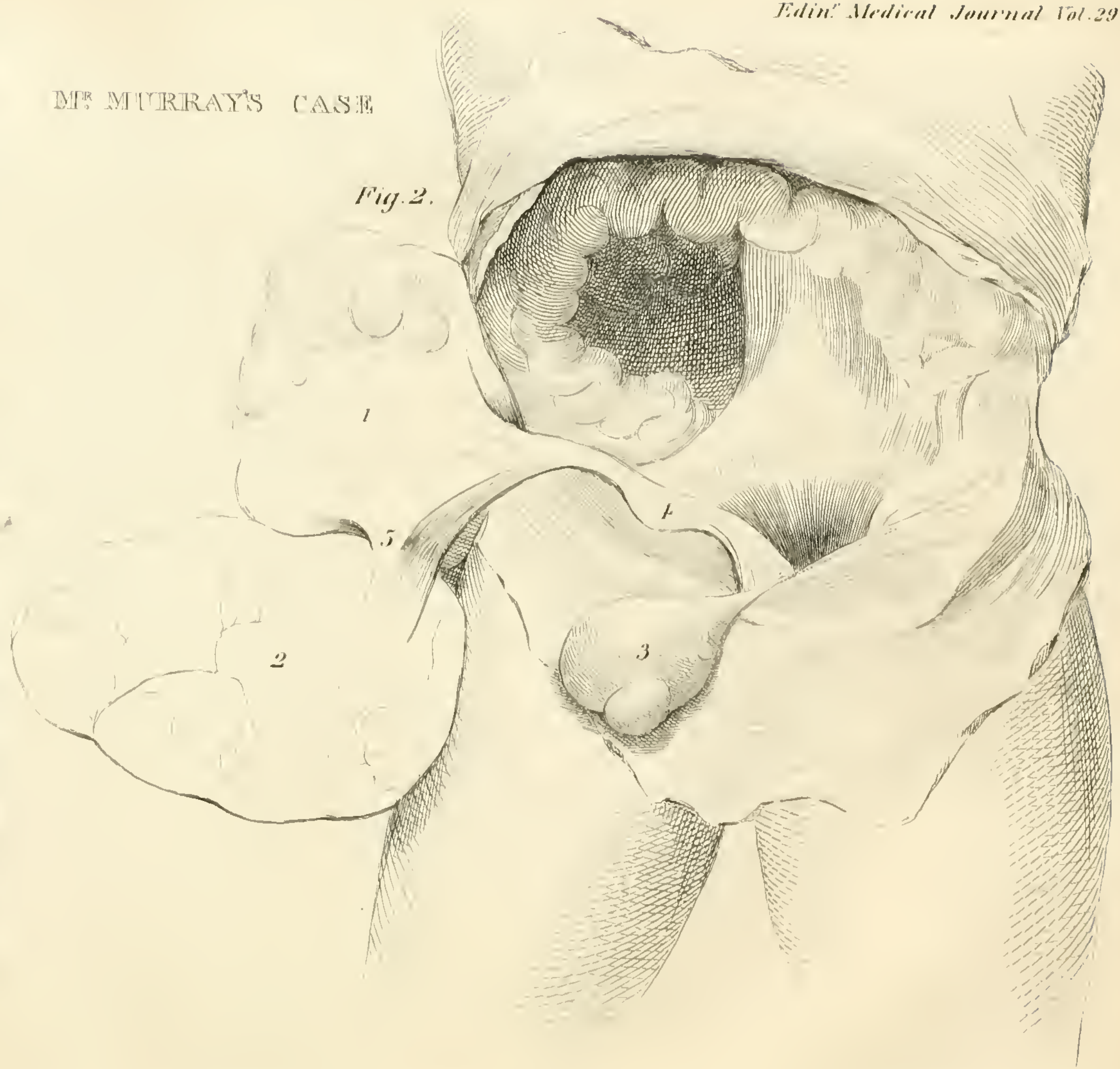


Fig. 2.



W.H.Lizars sc.

tempted with more confidence than in former times; and they must, I think, set the question of the propriety of extracting diseased ovaries at rest for ever.

In concluding, I would express my conviction that cases of this disease, admitting of successful interference of any kind, are rarely seen; but it is consolatory to reflect that the complaint is often, for a great length of time, without pain or danger, and does not necessarily prevent conception and safe delivery. In proof of this numerous instances could be adduced. In one of Morgagni's cases there was no bad symptom of any kind, even when the abdomen had extended to a span below the knee. In another instance, a very large swelling of this kind was inconvenient only by its bulk, and neither impeded conception nor delivery; and in a third, also by the same author, a person labouring under this complaint enjoyed perfect health. It appears indeed that in many instances the disease is more cumbrous than dangerous; and death takes place from other causes than disorganization of the ovaries. When, however, the pain or danger is very great, something *must* be done; and under all the circumstances, tapping is, I think, the step most advisable. When the tumour is in the pelvis and impedes delivery, to perforate it from the vagina becomes indispensable; but unless, for the above reason, it is probably never proper to open the tumour while in the pelvis; for though while there it often gives rise to troublesome symptoms, yet, as it usually soon enlarges and mounts up into the belly, where it is comparatively harmless, it appears best to wait for this natural relief.

Dr Coldstream of Leith, with his usual readiness to oblige, made from the parts the drawing sent along with this paper. In Figure 1 the right ovary (1), with its attached bladder (2), are seen, also the left ovary (3), with a peculiar knob at one corner. Below this lies the uterus (4.)

In Figure 2 the three tumours (marked as before 1, 2, 3) are seen as turned out from their situation. At 4 is the middle of the band going from the uterus to the right ovary, near the extremity of which are seen two vascular branches, one to the ovary, and the other to the attached bladder. At 5 is a membranous connection between the tumours 1 and 2. The dotted line shows the course of the left spermatic artery.

George Street, Edinburgh,
18*th February* 1828.

Art. XV.—*Case of Fungus Hæmatodes in the Kidneys.*

By Ebenezer Gairdner, M. D. Fellow of the Royal College of Physicians, Edinburgh.

Towards the end of September 1826, I was consulted by Mr and Mrs B. regarding the health of their daughter, Agnes, aged three years, who was considerably emaciated, and of a constitution deeply tainted with scrofula. She now laboured under some degree of febrile excitement. The tongue was whitish, with red point and edges, not dry; but it was said that she had great thirst. The bowels were slow, and the appetite impaired. The belly was peculiarly large and uniformly firm. On handling and pressing it there seemed to be no great tenderness or acute pain. The urine was not remarked by the parents to be any way peculiar, and it was reported to be frequently voided.

She had been nursed by her mother,—a fair, good-looking woman, about 28 years of age; and the child was considered to be healthy, until about two years ago when she was first observed not to thrive so well, and the belly to become rather full and bulky. She was also remarked to lose flesh, though the appetite was much greater than natural. Besides solid food, she would, in course of one day, have sometimes taken a quart bottle of porter; and she was often detected in picking plaster from the sides of the chimney and devouring it. Medical advice had been taken, but not for the last six months past. The size of the belly was remarked by the last gentleman in attendance, but no particular remarks were made as to the urinary secretions. The medicine he prescribed was hot in the mouth, and seemed to operate on the bowels. The parents objected to my proposal of his being again called in for consultation.

I ordered the bowels to be opened, and to be kept loose for some days by the frequent use of a powder containing five grains of calomel and two of prepared chalk; and for drink, cream of tartar-water with a little sugar. These had the effect of clearing the bowels of much fetid and lumpy feces, which were also greenish and slimy. The urinary discharges were also pretty free, but nothing peculiar was remarked by the attendants. The tepid bath, and frictions with the camphorated oil and a little ammonia, were enjoined; and beef-tea and nourishing food of easy digestion were recommended.

The febrile symptoms subsided in a few days, and soon afterwards there was some amendment of the general health. But the bulk and firmness of the belly remained nearly the same, and the morbid appetite for food and the craving for drink again returned. Towards the end of October, when her mother was seized with cephalitic fever, to which she soon fell a sacrifice, the child was frequently seen creeping and carried about, and thus brought under my notice. She seemed to suffer no pain, and continued to take regularly her food and drink with great avidity, and the alvine and urinary discharges were said to be pretty regular.

On the 16th of November notice was sent me that she had, during the preceding night, become worse. She had nausea, and a disposition to vomit; and, for the first time, the urine was remarked to be mixed with blood, and which I found was grumous and fluid. The pulse was small and feeble, and the extremities tended to be cold. Yet the child, lying on her back quite sensible and composed, with a countenance full of interest, gave no expression of much pain. Fomentations and cordials were applied; but she sunk in course of the day, without much apparent suffering or any convulsions.

The dissection was performed in the presence of several gentlemen; and both of the kidneys, on being removed, were particularly examined afterwards by Dr Aitken and his brother, Mr Thomas J. Aitken, in their dissecting room.

The body was much emaciated, but without ecchymosis. The abdomen was greatly tumefied, and on being struck it gave a dull sound. From the great tension, the *recti* muscles were considerably separated. The peritonæum contained a small quantity of fluid, but the omentum had some portion of fat. The stomach was concealed, being thrown behind, and the intestines pushed forwards and over to the right side, by what appeared to be a large tumour; displacing and carrying with it the spleen on its anterior and superior margin, likewise a portion of the duodenum and the pancreas on its inner surface, and which were firmly attached to it.

This tumour, occupying the left side of the abdominal cavity from the ilium upwards, was found to be the left kidney contained in its proper covering, but with the peritonæum adherent to it anteriorly. Its shape was an oblong rounded or oval, slightly flattened,—preserving in some degree that of the natural kidney, and at the lower end its appearance also. It measured ten inches in length, and fully sixteen inches in circumference, and weighed 5 lb. and 3℥ avoirdupois. It was, at the upper extremity especially, a fungous mass of a soft brain-like substance, with some little portions of grumous blood, but of firmer consistence towards the middle and lower end, with a number of cells containing reddish fluid of a urinous character. Some of these, bulging outwards, formed rounded irregularities on the surface of the kidney, and seemed to communicate with its pelvis. This cavity, or its infundibula, however, were not well defined, but led distinctly into the ureter, which was clear and open. There was no vestige of the medullary and papillary structure, but a mere membranous congeries, with cells and spongiform matter ; while at the lower end only of the organ a thin layer of the cortical portion remained. The emulgent vessels were traced from the aorta and cava ;—the vein was considerably enlarged ; but this, as well as the artery, were soon lost in the membranous texture forming the cells, and in the fungous mass.

The right kidney was found in its place, and to be also enlarged. It measured six inches in length and ten in circumference, and weighed 1 lb. and 3℥. Here also the medullary portion was much changed from its natural appearance and structure ; and the cortical portion was reduced in its substance, in some parts indeed almost wholly absorbed ; the upper end having somewhat the character of the *fungus hæmatodes*, while throughout, but especially in the middle and lower extremity, various cells containing blood and urine bulged out, forming globular projections on the external surface of the organ. The pelvis was large and pretty distinctly marked, as were also the ureter and vessels, and of the natural size. The intestines, somewhat inflated, were, as well as the mesentery, in a pretty sound state, unless that they were attached in some parts to the enlarged left kidney, as above remarked. The liver was quite natural in appearance and structure, though pressed up under the diaphragm, and completely concealed by the intestines. The gall-bladder was much distended with dark-green bile.

I was here led to adopt the active catharsis and the cream of tartar drink, not only to clear out the bowels, to allay thirst, and cool the system, but also with the view of increasing the urinary secretion, and enabling me to judge more accurately of the state of the abdominal viscera. My suspicions of either kidney being the seat of disease were, however, distant and vague until the latter end ; and they were perhaps rendered more so from having been informed, that the last medical attendant seemed not to have entertained any idea of disorder of these organs, if we may be allowed to judge from what the parents stated. But finding on dissection bloody urine not merely in the kidney that was so far advanced in disease, but also in the pelvis and cells of the one less affected, I am disposed to think that such urine may probably have been passed by the patient at much earlier periods, though not observed or duly noted. The necessity of remarking the urinary discharges I had frequently enjoined.

This is one of those cases, fortunately of rare occurrence, which presented no specific or well-marked symptom until near death, while both kidneys were involved in a most formidable disease.

Not that there are wanting in our records a number of similar cases in which the nature of the disease was not discovered till after death; but in these generally only one kidney was found affected, while the other was entire. In some of these cases the complaint has been ascribed to worms or tuberculated mesentery with tabes; in others to abdominal abscess or local dropsy; in others to calculus, or to disease of the bladder or urethra; while in others the spleen or liver has been supposed to be diseased; and in one case of a medical gentleman, the intestines or chylopoietic viscera were imagined to have been affected. From cases of this kind, it appears that in diseases of the kidneys the morbid discharges by urine, pain of loins, and vomiting, considered as the most unequivocal symptoms of derangement in their structure or functions, may not only be each and all absent, but may even be believed to indicate affection of some other organ. Upwards of seventy years ago, indeed, Morgagni declared, that he, as well as Valsalva, found the diagnosis of the diseases of the kidneys and urinary organs to be uncertain and fallacious. "Summa etiam arte quæsita, non rarissime fallax est," (Epis. xlii. 2, 3, 4, and 5.) In some cases they escape notice entirely. In others, in which the only symptom was vomiting, the stomach, rather than the kidneys, was supposed to be the organ affected; and in others this symptom was wanting till near the close of the disease. (Epis. xxx. 22. xl. 15. xlii. 14.) In the dissection of some cases one kidney is found to be decayed, and the other in consequence enlarged. (Ep. xl. 14.)

Baillie and others in our day have candidly acknowledged the same thing in scirrhus and some other chronic diseases of these organs, notwithstanding the advanced state of medical science, and the accumulated examples of morbid structure. The present case, moreover, confirms the observation of this eminent pathologist, "that the urine is secreted even when the natural structure of the kidney is almost entirely lost;" and "that a very small portion of the natural structure of the kidneys is capable of separating very nearly the ordinary quantity of the urine;" for there can be no doubt that not only had the right kidney maintained its office, but that even the left one had also preserved this in some degree, as the reddish fluid found in its cells had the sensible qualities of urine.

Art. XVI.—*Remarks on Strumous Corneitis and Iritis, with Cases.* By James Guthrie, Esq. Surgeon, Kilmarnock.

The ophthalmia to which I have given the above designation occurs most frequently in girls of a strumous habit, about the age of fourteen, in whom the menses have not appeared.

The inflammation is slow and insidious in its progress, marked by a very slight redness of the conjunctiva, and the greatest possible degree of intolerance. The vessels on the sclerotica are seen on close inspection very minutely injected, and running in straight lines to the margin of the cornea, where they form the well known appearance of a vascular zone. The cornea soon becomes opaque; the iris contracted; and the aqueous humour muddy. A profuse gush of scalding tears follows the opening of the eye. Though the sensibility is morbidly increased, the patient rarely complains of pain either in the eye or head, the aversion to light being the principal distress.

The constitutional symptoms run very high; the pulse becomes quick; the appetite is lost; and the bowels constipated or otherwise deranged; skin harsh and dry; and the whole system in a high state of irritation.

I have not been able to detect any very evident exciting cause. The disease occurs most frequently in the months of April, May, and June; and invariably in those of a strumous irritable habit. Girls are most liable to it, particularly about the age of fourteen. As an exception to this remark, I lately saw a well-marked case in a woman forty years of age, in whom the menses were suppressed. The sight was irrecoverably lost before the patient consulted me. The case ended in a leucomatous state of the corneal texture, with opaque capsule, to which the pupillary margin of the iris adhered by a pencil of lymph.

The inflammation has its seat principally in the cornea and iris, both of which are affected simultaneously; and where the whole of the morbid action is expended, the other textures no doubt participate in the inflammation, particularly the choroid; and although the evidence on this point is rather deficient, its existence, I think, is indisputable.

The disease is extremely obstinate, running a course of from two to three months, yet, when well managed, rarely produces permanent injury to vision. The cornea, though apparently disorganized, resumes its transparency, and the iris its functions, under the influence of those remedies which subdue the inflammation and photophobia. But when allowed to run on uncontrolled, it terminates in coadhesion of the iris and cornea; constricted pupil; opaque capsule, with partial adhesion thereto of the pupillary margin of the iris. If the anterior chamber is preserved, a leucomatous state of the cornea is a frequent result.

Treatment.—The mild, indolent character of the disease, renders it more difficult to subdue than a fixed and vigorous action; and the practitioner is insensibly betrayed into irresolution by the seeming inertness of the disease and the obvious delicacy of the patient. In this kind of ophthalmia I have never found the detraction of blood, either local or general, of any avail. The cases in which it was employed turned out the most obstinate and intractable. Leeches are not much less debilitating than the lancet, owing perhaps to the local irritation thereby induced. Syncope, in such cases, from dread of the lancet, is the best substitute for the detraction of blood. The principle of treatment indicated in such cases is to lessen the irritability of the system, without materially depressing its power. This object is most effectually obtained by the administration of emetics, which should be repeated every second or third day for the first week. We must, at the same time, exhibit ten or twelve grains of Dover's powder every night, to be followed up next morning by a small dose of sulphate of magnesia, to regulate the bowels and prevent the constipating effects of the opium.

This treatment will soon give a turn to the inflammation, and reduce the morbid irritability. This being accomplished, mercury must be had recourse to, and introduced into the system, so as to disturb it in the least possible degree. Profuse salivation is not wanted. Its effects are often very decisive long before the gums become affected. Mercury in this disease is a remedy of great value, and will seldom disappoint us, if not too early employed. I erred myself at one time in this particular. Until the irritability of the system is reduced by the means pointed out mercury is injurious. This fact I have had many opportunities of establishing; but when seasonably employed I know of no remedy competent to the same effects. Blisters are not useful. The benefit expected from them is altogether counterbalanced by the irritation they produce.

Belladonna ranks next in importance to mercury; without it the case could not be brought to a favourable termination. The extract should be thickly painted on the eyebrow and eyelid every night at bed-time. Besides its influence over the pupil, which is very limited for some time, it has a considerable effect in allaying the local irritability.

Stimulating drops and ointments must be discarded altogether during the acute stage, as well as every other doubtful remedy. The only local application from which I have seen any benefit is the steam of warm water containing laudanum. From this the patient generally derives great relief.

When the inflammation has been considerably reduced, and the intolerance of light removed, a little weak precipitate ointment introduced into the eyes twice a-week will be of great service in restoring the cornea to transparency. Solutions of nitrate of silver, or muriate of mercury, or *Vinum Opii* may be occasionally interposed.

In the convalescent state tonics and sea air are required to complete the cure and re-establish the health. Should the disease lapse into a chronic form, a termination which I have occasionally seen, caustic issues superadded to the local treatment will be found successful.

I now proceed to the detail of two cases. I have selected the following as being most illustrative of the foregoing observations.

Case I.—Margaret G. aged 14, of a strumous habit and delicate constitution, has been affected with inflammation in the left eye for a fortnight. The symptoms are the following. Redness of the eyeball apparently trifling; close inspection, however, discovers the vessels on the sclerotica very minutely injected, and running in radii to the margin of the cornea, where they form a zonular appearance. The cornea is hazy, and its serous vessels loaded; iris contracted; intolerance of light excessive; lachrymation, and vision is much impaired. She makes no complaint of pain either in eye or head; pulse small and quick; and the constitutional disturbance very considerable. Menstruation has not commenced.

The following treatment was in the first instance adopted: leeches repeatedly applied to the temple, and a suppurating surface established behind the ear. The bowels being cleared by a purgative, calomel in combination with opium was given twice daily. A solution of nitrate of silver was applied to the eye and extract of belladonna to the eyelid every night. The above treatment was persevered in for four weeks, without in the least interrupting the progress of the disease. The mercury had in the interval affected the gums, and was given up. The cornea is now rendered opaque, and very much thickened, presenting in one spot a circumscribed deposit of lymph. At this part the cornea has an elevated appearance. Iris extremely contracted and irregular, but with difficulty seen; aqueous humour muddy. The pupil resists the influence of the belladonna; aversion to light unabated; vision gone. The right eye has taken on the same morbid action. The constitutional disturbance is now so great as to confine the patient to bed. The irritation in the system (I mean its sympathy with the local disease,) appears to play the most important part in keeping up the disease; to subdue which emetics every second day, Dover's powders in doses of ten grains every night, and half an ounce of Epsom salts every morning, were ordered. The good effects of this treatment soon became apparent. The symptomatic fever was wholly subdued, and the morbid changes in the cornea arrested. At this period mercury was again had recourse to. The amendment was now progressive. The restoration of the cornea to transparency, and the recovery of vision, were rapid and complete beyond all expectation. Red precipitate ointment was interposed to assist the restoration of the cornea.

Case II.—Mary L., aged 13, of a scrofulous habit, has been ill with ophthalmia in left eye for ten days. The following symptoms were observed. The vessels in the sclerotica minutely injected; a vascular zone beginning to form at the verge of the cornea; pupil extremely contracted; aqueous humour muddy; cornea slightly opaque; vision very dim; aversion to light at present inconsiderable; no pain in eye or head.

The following remedies were actively employed:—V. S. *ad ℥xij.*; nauseating doses of tartrate of antimony; saline purgatives; blister behind the ear, kept running; the steam of warm water containing laudanum to the eye; belladonna to the eyelid; leeches were also repeatedly applied to the temple; and finally, calomel and opium twice daily. Notwithstanding these means, the inflammation continued its progress; and in the course of four or five weeks the cornea was rendered completely opaque. The pupil cannot be seen; vision entirely gone; intolerance of light excessive; symptomatic fever very high. The right eye has become affected. Mercurial inunction was superadded to the calomel and opium, and in eight days salivation was established. This, so anxiously looked for, entirely disappointed me. At this period the last affected eye presented a most alarming appearance. The cornea at its junction with the sclerotica was of an intense red colour. This appearance extended some lines over the corneal margin. The cornea was very much thickened, and red vessels were seen ramifying over its entire surface. The iris was entirely obscured; aversion to light urgent; vision in both eyes was quite extinguished. Sarsaparilla was substituted for the mercury. And to subdue the irritability of the system, emetics, Dover's powder, and saline laxatives, were ordered, as in the former case.

These means very soon interrupted the march of the disease. As soon as this was accomplished I again commenced a mercurial course; and although several days elapsed before the constitutional effects of the mineral appeared, its effects were most decisive. The improvement continued progressive. Red precipitate ointment and *Vin. Opii* were applied to the eyes; and in a short time the cornea was restored to transparency, and a perfect state of vision obtained.

When the acute symptoms began to yield, the conjunctiva, which had all along taken so little share in the inflammation, became highly vascular. There remained a faint appearance of the vascular zone at the verge of the cornea for a considerable time, which, however, gradually disappeared.

Having met with this case not long after the first, I had still to learn the obstinacy of the disease under the ordinary treatment.

When this case presented itself, I resolved on pushing two of the most powerful remedies to their fullest extent; and what was the result?—The loss of blood, by reducing the system, rendered the case almost irremediable. The too early use of mercury, on the other hand, hurried on the disease, and increased the irritation to an alarming extent.

Kilmarnock, 25th August 1827.

Art. XVII.—*Case of Laborious Parturition, complicated with Laceration of the Uterus and Vagina.* By William Campbell, M. D., Lecturer on the Practice of Physic and Midwifery, &c.

Mrs Ferguson, æt. 36, of rather spare habit, the mother of seven children, was seized with uterine action on the 21st of November 1824, at 9 p. m., while in the early part of the last month of this her eighth pregnancy. Pains continued until between one and two in the morning of the 22d, when they suddenly, almost entirely, ceased. After the cessation of uterine action, the pulse became extremely rapid, small, and indistinct, accompanied by a trifling effusion of blood *per vaginam.*

The patient was under the care of my friend Mr James Scott of Broughton Street; and I saw her about an hour after the character of the pains had become changed. She was then in a most alarming state. The abdomen could not bear the slightest pressure without violent pain being produced; the pulse was too quick and indistinct to be numbered; the countenance pale and collapsed; considerable thirst was complained of; and there was a disposition to bear down, but without effect. Immediate delivery was resolved upon, and, to accomplish this, forceps were applied over the head, which was placed upon the brim, but from its great size, it could not, after a cautious trial of half an hour, be brought through. The forceps had nearly slipped off the head during the attempts of extraction, from the elasticity of its bones, whereby we were induced to think that the cranium contained water. The forceps was withdrawn, and the perforator pushed into the anterior fontanelle, which gave exit to a considerable discharge of water; and this was soon followed by the expulsion of the fœtus, and about 2 lbs. of coagulated blood. As the uterus was in a state of relaxation, and blood continued to be effused *per vaginam,* the hand was introduced into the passages, when an extensive rent was discovered in the upper and back part of the vagina, extending through the cervix to the lower part of the body of the uterus. The hand passed through this opening, among the intestines, before we were scarcely aware of the accident. The pelvis was well formed; and the patient had an easy time in all her previous labours.

At half-past one P. M. vomiting of dark coffee-coloured matter commenced, and the quantity brought up in the course of the day amounted to about 2 lbs. While in this condition, Professor Duncan Jun. saw the patient, and agreed with Mr Scott and myself in thinking her in a most unfavourable state. The pulse ranged from 130 to 150 in a minute, and the lower extremities occasionally felt cold. Beef-tea and port wine, in small quantities, were allowed for support, and bottles of hot water were applied to the lower limbs. The patient had four enemata in the course of the day, each containing a pound of beef-tea and ten drops of the tincture of opium. 23d, 10 A. M. Pulse much improved in firmness and regularity, and beat 126. There were occasional pains in the abdomen; lochia moderate; urine to the amount of six ounces was drawn off last night by the catheter. 6 P. M. Black vomit reappeared, and continued until about six pounds of it were brought up. The bladder was evacuated several times in the course of the day with ease. The passages were washed out with about twelve ounces of warm water; and the following draught was ordered at bed-time.

℞ *Solut. opii sedativ. gt. xj. aq. menth. ℥j. M.*

24th, Pulse as yesterday; no vomiting since last night, though a considerable quantity of beef-tea has been taken by the mouth. The patient had a very fœtid alvine evacuation this morning, and she complains of considerable pain and tension of the abdomen. At this visit she was ordered a pound of gruel and two ounces of the volatile oil of turpentine for an enema; to have the abdomen fomented by flannel compresses wrung out of one pound of warm water and four ounces volatile oil of turpentine. 6 P. M. Enema procured three copious evacuations of feces and flatus, which were followed by great relief. An examination was made *per vaginam* at this visit, and the situation of the rent in the posterior part of the *cervix uteri*, extending towards the promontory of the sacrum, could be traced as far as the fingers could be advanced. There was no visceral protrusion, and the uterus did not seem at all sensible, as the patient scarcely complained of any uneasiness during the examination.

Haust. ut heri, nocti hora somni habeat.

Nov. 25th.—Patient had a good night; pulse 114, firm and regular; tongue and skin moist; no disposition to vomit for the last two days; tension of the abdomen less; had an alvine dejection in the morning. Beef-tea and wine were ordered to be discontinued, and a little panado substituted.

Haust. ut heri nocte rept.

26th.—Stools have been thin and frequent, accompanied with a considerable degree of tormina and tenesmus since the exhibition of the turpentine enema.

Habeat statim Ol. Ricini ℥ss.

She was ordered a little rice boiled in milk, for nourishment.

27th.—Pulse 112; skin temperate; diarrhœa going on.

℞ *Confect. Aromat.* ʒ*ij, Tinc. Kino* ℥*j, Tinc. Opii, gt. xl; Tere bene simul, et postea gradatim adde, aq. cinnam. et aq. pur. ana* ℥*ijss. Fiat haustus, cujus ægra cochleare amplum, quæque tertia hora sumat.*

To be allowed a little chicken for nourishment.

29th.—Pulse 102; mammæ a little tense and painful. Patient continued to regain strength, and we gave up visiting her on December 2d. She remained long delicate, and has not conceived since. During the whole of this pregnancy her feelings were very different to what they had been on former occasions; she felt herself weakly, heavier, and not so active as in her former pregnancies.

The fœtus, independent of its being rather small and hydrocephalic, had *spina bifida* on the lumbar portion of the spine, and its lower extremities were much distorted. The cranium of this fœtus, which is now among my collection, is to a great extent membranous. Its circumference from the chin to the vertex is 11 inches; from the chin to central portion of the sagittal suture $8\frac{5}{8}$ inches; from the root of the nose to the vertex $9\frac{3}{8}$ inches; and from one parietal protuberance to the other $8\frac{5}{8}$ inches.

4, *Picardy Place, Feb.* 27, 1828.

ART. XVIII.—*On the Efficacy of Spurred Rye in inducing Uterine Action.* By ROBERT RENTON, M.D. Fellow of the Royal College of Physicians of Edinburgh, and Physician to the New Town Lying-in Institution.

THE supposed efficacy of horned rye in increasing the action of the uterus has lately produced in this country considerable discussion, particularly in that branch of the profession devoted to obstetric practice; and if this substance be found to realize in part only the high expectations entertained of it by its advocates in America and this country, its virtues cannot too soon be made known to every practitioner of midwifery.

In all the trials of its reputed efficacy as a physiological agent, in the cases hitherto recorded, it cannot be denied, I imagine, that a perplexing source of fallacy is found in the following circumstances. Contraction is at a certain period natural to the impregnated uterus; and takes place whether means be used to favour or to counteract it. In different females, and under different circumstances in the same woman, this contraction may be either retarded or accelerated in point of time, or it may be weaker or stronger in degree. Every practitioner in midwifery is aware how much these contractions are influenced by the state of the general health, and by those causes also which operate on the mind of the individual. We know that mental distress and the depressing passions in general retard and diminish uterine action; while those of an opposite tendency, which powerfully operate on the mind, do accelerate and render it more energetic. It is matter of daily observation that confidence in the efficacy of any means tried may be followed by more powerful and efficient uterine action.

When spurred rye is given, as it generally is under these circumstances, the difficulty of ascertaining the exact effect which it produces is very great. It is possible that the result which follows is a coincidence, not an effect. In some cases even imaginary confidence in its powers may be the real cause of the increase of uterine action. In others, again, it is difficult to say how far the action of the uterus, which operates periodically, might or might not be influenced by the substance used.

Accordingly I, in common with many other practitioners, attributed the supposed success of the ergot in some cases to the renovated powers of nature; in other instances to the high excitation of mind occasioned by the hope of a speedy termination being made to the suffering of the patient, and which hope would, in some degree, be commensurate to the faith reposed in its virtues by the practitioner. In some cases which came under my notice, but which were entrusted to the care of gentlemen acting as assistants to the New Town Lying-in Institution, in which the rye had been used, and supposed to have succeeded in rousing the dormant energy of the uterus, this explanation appeared to me more satisfactory, than to believe that it should exert a specific influence on the uterus. I have not, however, been dissuaded from making trials of it in those cases in which there appeared to be not only a deficiency of energy in the uterus, but of the general system, and I think with good effect. I would not, however, consider myself warranted in publishing the history of these cases as proofs of the usefulness of the ergot; yet I have no hesitation in admitting, that the impression left on my mind of its apparent efficacy, in these instances, had no small weight in recommending its employment in the following case. I adduce it as one in which there was, so far as I can judge, less room for the operation of any of the above-mentioned sources of fallacy than in any other of which I am aware.

On Sunday, the 13th January 1828, I was requested by Dr Moncreiff to visit a Mrs G. aged 30, the mother of two children, supposed to be ill of fever, but who had that morning at 8 A. M. given birth to a fœtus between the third and fourth month.

The placenta had not been expelled at 3 P. M. when I first saw her, and it was in consequence of its detention, having created alarm in the mind of the patient and her friends, that my attendance was requested. From particular circumstances, I found it impossible to make an examination, so that no correct opinion could be formed of the real situation of the placenta. As the patient was of a very spare and feeble habit, and seemed much exhausted from fatigue as well as flooding, I recommended the adoption of the usual means in such cases, and took my leave. At my visit on Monday I found her somewhat refreshed; she had passed a good night; had suffered no pains; and the discharge had abated in some degree. On examination I found the anterior part of the *cervix uteri* much distended, which I attributed to the presence of the placenta or coagula, or of both. The placenta was found situated within the *os uteri*, but from the impossibility experienced in attempting to dislodge and extract it, I was satisfied that an adhesion existed between it and the anterior part of the *cervix uteri*. As the patient had complained of few after-pains, and of none three hours after the expulsion of the fœtus, the inactivity of the uterus, in addition to the adhesion of the placenta, was obviously an indirect cause of the detention of the afterbirth. In order to induce uterine contraction, stimulating clysters were recommended to be thrown up the rectum, and moderate pressure applied to the uterus by means of bandaging round the body. These means were persevered in during Tuesday and Wednesday without having produced the least effect on the uterus. As there were good grounds for apprehending a sinking of the general system, partly from the drain that had been kept up for several days, notwithstanding the means adopted to counteract it, as also from the constitutional mischief likely to arise from the presence of a putrid placenta in the uterus, the signs of which had now become very evident, I regarded the case as a most favourable one for subjecting the *Secale cornutum* to the test of experiment. On Wednesday evening a drachm of it was infused in four ounces of boiling water for half an hour, the one-half of which was directed to be taken at half-past nine, and the remainder in an hour, if no pains occurred. Both doses were taken without having had apparently any effect. In rather less than half an hour after swallowing the last dose uterine contraction commenced, which continued with intervals of about two minutes between the pains, till 6 A. M. the following morning, each contraction having been accompanied and followed by sanguineous discharge, both in a fluid and coagulated form. The patient complained of the severity of the pains, and represented them as violent as those she had experienced during her former confinements. The greater portion of the placenta had been expelled during the night; but on examination a part still was found adhering to the uterus. On explaining to her the object in giving her the medicine, she agreed to take another dose in the evening, if the remaining portion of the placenta should not be discharged. This was, however, not found necessary; for in the effort of pressing at stool in the course of the evening, the remainder of the putrid diseased placenta was expelled. After which her recovery became progressive, and is now complete.

Two facts worthy of remark present themselves. 1*st*, That the uterus had given no evidence, either to the patient or her attendants, of any disposition to expel its contents later than three hours after the expulsion of the fœtus. 2*d*, That within half an hour after the last dose of the rye, uterine contraction ensued, which continued for the space of seven hours,—a most unusual occurrence in such cases, particularly in feeble constitutions, rendered more so by a protracted flooding.

29, *James's Square*, 26*th Jan.* 1828.

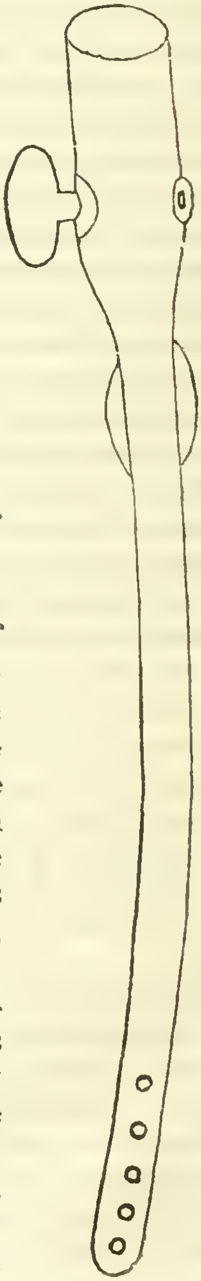
ART. XIX.—*Suggestions for the Improvement of the Female Catheter.* By WILLIAM F. MONTGOMERY, A. M., M. B., Professor of Midwifery in the King and Queen's College of Physicians in Ireland.

THE introduction of a catheter into the female bladder, and the evacuation of the urine by its means, is confessedly a very simple operation; but as it is also an operation from its nature very revolting to female delicacy, and regarded by women in general as much more simple than it really is, they are apt to be much influenced in their feelings by the neatness and dexterity with which it is performed.

If I had no other reasons than these for the suggestions which I have to propose for the adoption of the profession, I should consider them fully sufficient to warrant me in so doing. But I am inclined to hope that some consideration of higher importance will be found connected with the alteration in the construction of this instrument, which I have to recommend from experience of its advantages.

The female catheter in general use in this country has no other provision for preventing the instantaneous flow of the water, as soon as its point enters the bladder, than the insertion of a plug attached to the stilette. This plug is hardly ever fitted with such accuracy, even at first, as to prevent the escape of the urine from the end of the instrument before a vessel can in many instances be placed to receive it. This is a defect of construction which is made a subject of very general complaint, as causing on ordinary occasions a disagreeable inconvenience, and which, under particular circumstances, may be productive not only of very considerable annoyance, but even serious injury to the patient.

Fig. 1.



To remedy this defect in the first place, and with other intentions to be afterwards noticed, I wish to propose, instead of the plug, the adaptation of a stopcock (as represented in the annexed sketch, Fig. 1,) by means of which we are enabled completely to prevent the escape of a drop of urine until we are prepared to receive it.

In cases of retention of urine occurring under circumstances which preclude the possibility of repeated visits from the medical attendant, the use of an instrument so constructed will enable the patient herself or an attendant to let off the contents of the bladder in the intervals of the physician's visits, the instrument having been previously secured in the urethra by passing the tapes of a bandage through the eyes or loops on its sides, and interposing a soft thin cushion, through which the catheter should pass.

I should here observe that it is not necessary that the catheter should be of metal, as the metallic bell containing the stopcock may be securely attached to a portion of the flexible or common gum elastic catheter.

Another inconvenience experienced even in common cases is the application of a vessel to receive the water. We cannot always prevail or insist on a patient to assume the position best suited to our purpose. Change of posture may be prevented by pain, or equally by the excessive confusion of shame with which she is overwhelmed at the idea of the operation. Then the vessels usually met with in bed-chambers are, from their height and size, particularly unfit and difficult to apply readily under the extremity of the catheter, and their apposition very apt to be attended with some degree of exposure, if the patient be not placed completely over the edge of the bed. Again, in the removal of such vessels it very constantly happens that some of the urine is spilled into the bed, or that the bed-clothes fall into the vessel, in either case causing much discomfort to the patient.

Fig. 2.

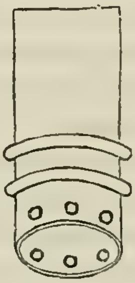


Fig. 3.

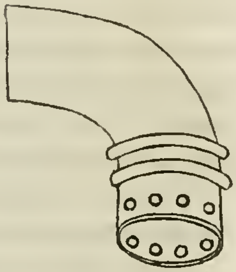
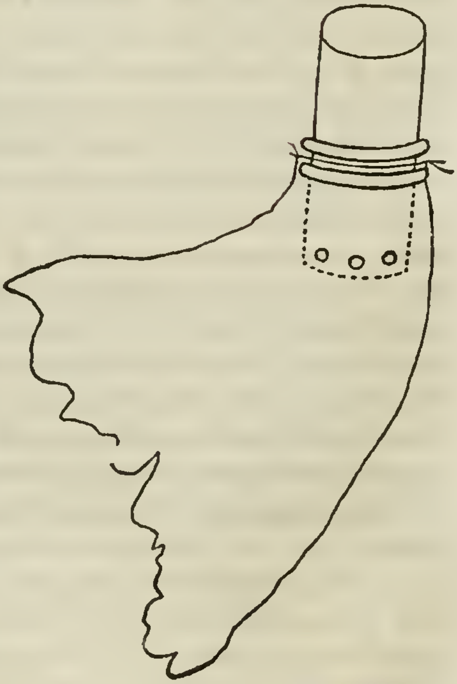


Fig. 4.



In order to obviate such inconveniences, and to avoid the necessity of having to seek any receiver, I have adapted to the bell of the catheter a silver cap, (see Fig. 2 and 3,) to which is attached a fine bladder, (see Fig. 4,) such as that of a calf or sheep, prepared for the purpose by moistening it a little with oil. An instrument provided with this addition is not in the slightest degree less manageable than when used without it. The bladder lies in the palm of the hand, without in the least interfering with the management of the instrument by the fingers; or the catheter may be first introduced, and the cap afterwards slipped on, without any difficulty or delay. At the same instant the stopcock may be turned, and the urine is discharged into the attached bladder, without further disturbance of the patient; without even the apprehension of exposure on her part; and without her experiencing what, under some circumstances, might be a matter of no small importance, the feeling of cold, either from air admitted in removing the bed-clothes, to allow the introduction of a large vessel, or from the chilling contact of the vessel itself.

The circumstances of a case every day met with in practice will perhaps best illustrate the advantages to be derived from the use of an instrument so provided. We are called to see a patient with retention of urine, accompanying perhaps the partial paralysis, which is sometimes a consequence of severe labour, or arising from some severe accident, or the effect of fever. We find such a patient lying sunk in the middle of a large bed, totally helpless and incapable of turning on either side, or even of being turned without great disturbance or suffering. In a case such as this, with the patient so situate, if a catheter be introduced, its end is so much depressed towards the bed, that we cannot apply under it any vessel, commonly to be had in the sick-room; and in order to make the attempt, the limbs of the patient must be either elevated by an attendant, or considerably separated. Here is incurred the risk of striking the vessel against the end of the catheter and causing pain, or at least irritation; or spilling the fluid, an accident which causes serious inconvenience.

These objections, I think, are completely obviated by using an instrument constructed as I have advised.

I hoped at first that this instrument might be found of service in those cases of incontinence of urine which are caused by sloughing of the neck of the bladder from pressure during labour; but the urine escapes between the catheter and the edges of the aperture; at least it did so in the only case in which I had an opportunity of trying it. But a case might perhaps occur in which the breach would be so small as to be sufficiently commanded by the instrument, though in such cases I fear little can be expected from it. In some other cases of incontinence, however, its use will, I hope, be found peculiarly suitable. Other applications of the contrivance may occur to other practitioners; and an intelligent friend suggests that it might be conveniently used in the operation of injecting the bladder.

It will be found convenient to have the cap perforated with six or eight holes round the end which projects into the attached bladder; that, in case of the bladder lying on and stopping the large aperture, the water may escape by the lateral openings.

Cuffe Street, December, 4, 1827.

PART II.

CRITICAL ANALYSIS.

Art. I.—*Pathological and Practical Researches on the Diseases of the Brain and the Spinal Chord.* By John Abercrombie, M. D. Fellow of the Royal College of Physicians, Edinburgh, &c. Edinburgh, 1828. 8vo, pp. 444.

To the readers of this Journal the researches of Dr Abercrombie have been long well known; and few who take any interest in the progress of pathology can be unaware of the assiduity and perseverance with which he has cultivated this branch of medical knowledge. During several years he has continued to lay before the profession, chiefly through the medium of this Journal, a series of researches on various subjects in the pathology of the internal organs of the three cavities. Of these it is unnecessary in this place to speak in the language of panegyric. If they were entitled to no higher commendation than that of a useful and well digested record of pathological and practical facts, their merit would be great indeed. We know that we express only the opinion of the most judicious part of the profession, when we say, that to the great progress made by pathological science of late years, they have contributed in no ordinary degree.

Though the researches of Dr Abercrombie have not been confined to one cavity or one organ, but have been directed to all those organs and textures for the unsound states of which the advice of the physician may be requested,—those incident to the brain, *cerebellum*, spinal chord, and their membranous coverings, claimed a large portion of inquiry and observation. In several essays published in the 14th and 15th volumes of this Journal, Dr Abercrombie took a survey of the morbid states incident to these parts, under the three heads of inflammatory disorders, apoplectic and paralytic affections, and organic changes. Of such inquiries the chief object is to define the pathological characters of each morbid state, to give a connected view of the mutual relations which almost invariably exist among them, and to trace, if possible, between them and their external effects or symptoms, such connection as may enable the physician to recognize their existence, and to manage them with success. This it was the purpose of Dr Abercrombie to accomplish; and it has been generally admitted, that, allowing for the inherent difficulties of the task, his attempt, though the first, has been the most successful.

Various circumstances, indeed, well known to the physiologist and physician, tend to involve this subject in the deepest obscurity, and to invest it with difficulties, which nothing but the most assiduous and accurate observation, conjoined with sound judgment and patient inquiry, can ever hope to surmount. To say that these difficulties are partly, if not chiefly the result of erroneous observation and false theory, does not in the smallest degree render the task of inquiry less arduous. It may even occasionally add to the labour, by giving matters of fact an adventitious colouring, and placing them before the eye of the observer in a false and distorting medium. To expect an undertaking of this description to be entirely successful would be unreasonable indeed. The essays, therefore, already alluded to, though possessed of very great merit, partook necessarily of some of those deficiencies and imperfections from which first attempts can never be wholly exempt. To supply such deficiencies, and correct such imperfections, is the chief object of the work now brought before the attention of the reader.

It is natural to expect that a work published under such circumstances must present many points, not only of coincidence and resemblance, but of perfect identity with the essays in the form of which its essential contents originally appeared. Though this is in truth the case to a great degree, it is not nevertheless to such an extent as to render the present volume entirely similar to his early productions. Were this the case, our task would be easy; and all that it was requisite to say might be comprised in a few words. It is due, however, to Dr Abercrombie to say, that on many points he has modified his inferences; on several he has taken occasion to correct them; and on all he has extended his researches and inquiries so much as to exhibit a more elaborate and connected, and perhaps a more just view of the several divisions of the subject of investigation. Having premised these observations, it is competent for us to say, that, though we feel it unnecessary to enter so much into the details of the present work, as if it had never in any shape been before the medical reader; yet, to preserve uniformity, avoid confusion, and do justice to the author, it shall be our study, without more allusion to the first essays of Dr Abercrombie than is absolutely necessary, to give a succinct account of the principal contents of the present volume, and the most important pathological results to which the inquiries of Dr Abercrombie lead.

In a short but well-written preface the difficulties of the subject are stated, and the necessity of founding general principles on extensive collections of accurate and well-established facts only, is fully admitted. Strongly impressed by these considerations, while the ingenious author acknowledges that he supports no system, and proposes no new doctrines, he modestly disclaims any other merit than that of collecting and arranging facts; and whatever conjectures he offers either to explain their nature, or account for their origin, are studiously separated, that the real value of the former may not be impaired by admixture with the latter.

In the present work, as in the original essays, the entire subject of diseases of the brain and its membranes is considered under the three general heads of inflammatory affections, apoplectic affections, and organic affections; while a fourth part consists of diseases of the spinal chord. We must presume that the author finds this the most convenient arrangement for his purpose; for it is obvious that it is not entitled to the character of being very clear or distinct. It is an objection of some force, that inflammatory affections of the brain or its membranes not only produce, but often terminate in apoplectic symptoms; that apoplexy itself is, pathologically speaking, very often an inflammatory disorder; and that almost all the organic changes incident to the brain are accompanied or followed by more or less inflammation, either chronic or acute, and almost invariably terminate in apoplectic death. It is further no inconsiderable objection to this division that the principle on which it is made is not uniform, or of general application to its several parts. Thus the first part, or that of inflammatory disorders, is derived from pathological characters; the second, the apoplectic, from symptomatic characters; while the third again is taken from the physical changes which the structure of the parts undergoes. In the fourth part a new principle of division is again employed, in so far as the anatomical situation of the part is selected as the general head, which is again subdivided into a considerable number of sections, partly on pathological, partly on symptomatic, partly on organic characters.

Of these objections the ingenious author doubtless was aware, and would have obviated them if he had thought it important to do so, or had it been consistent with the general plan of his work. They serve to show rather the difficulty of the subject itself than any imperfection in the manner in which it is treated. It may further be remarked, that the errors of the mode of division are compensated, so far as is practicable, by the distinctness with which the subordinate divisions are treated, and by the instructive matter which is every where brought forward to illustrate the several forms of cerebral or meningeal disease. No distinction is made, no pathological position established, unless on the evidence of facts competent, according to the laws of sound reasoning, to uphold its truth.

After a general view of the symptoms which indicate inflammatory disease within the head, Dr Abercrombie proceeds to consider the principal seats and terminations of the disease. These terminations vary manifestly according to the texture of the part affected; but with the exception of one form, that of pulpy destruction *(ramollissement,)* Dr Abercrombie does not attempt to distinguish the terminations according to this principle. Thus, inflammation affecting the textures within the scull, may be fatal in the inflammatory stage, whether seated in the membranes, or in the cerebral substance, though the most distinct cases of this description take place in the centre of the hemispheres. Serous effusion again, which is admitted to be the second mode of termination, may take place within the ventricles, in the subarachnoid cellular tissue, and between the arachnoid membrane and *dura mater*. Deposition of false membrane, the third mode of termination, is most common beneath the arachnoid membrane, and is occasionally seen covering the choroid plexus within the ventricles. Suppuration, a fourth result, though often found beneath and occasionally above the arachnoid, is most usual, however, in the substance of the brain, either in distinct abscesses bounded by cysts, or in irregular broken down cavities. A fifth result, pulpy disorganization, is regarded as analogous to gangrene in other parts of the body; and a sixth head consists of changes in the thickness of the membranes, contraction and obliteration of the sinuses, and caries of the bones, which are known to take place in consequence of certain *chronic* forms of the disease.

This mode of distinguishing the several terminations of the inflammatory process, when seated within the cavity of the cranium, is obviously more practical than pathological; and perhaps it may be said there is no sufficient reason why it should not be the former rather than the latter. It must be presumed, that under all the circumstances it appeared to the author the most convenient which he could adopt; since his long habits of accurate observation and much reflection on the objects of that observation could not fail to apprise him not only of its objections, but of its advantages. It would be idle to deny that it is free from objections, not the least of which is, that it is deficient in precision, in so far as it arranges under the same general head, different morbid states and processes going on in the membranes alone, or the brain alone, and which, though occasionally found co-existing, are nevertheless very often separate and distinct. It is not our intention, however, to enter into minute criticism of the propriety of this mode of viewing the subject. It will be more useful to proceed at once to consider the subject in its minute ramifications. This is done chiefly by illustrating his general positions by appropriate and instructive examples of each form of disease.

The first which claims attention is inflammation of the *dura mater*. The rarity of this as an idiopathic affection is demonstrated by the circumstances of one case in which purulent matter was collected between the bone and *dura mater*, with marks of extensive inflammation of the arachnoid membrane. As an effect of external injury, it is greatly more common. But in practice it is often associated with disease of the ear and petrous portion of the temporal bone; and to this Dr Abercrombie justly directs much of the attention of the reader, and illustrates the nature of the disorder by three apposite cases. He properly represents it to be attended with caries of the petrous portion of the temporal bone, detachment, thickening, and ulceration of the *dura mater*, deposition of purulent matter and lymph, and superficial suppuration of the brain or cerebellum. In a few rare cases it produces suppuration of the substance of the latter.

The connection between disease of the ear and that of the brain and its membranes was early noticed by Ballonius, Bonetus, Morgagni, and Gontard; and though cases of this nature have since been made the subject of consideration by Mr Brodie, Mr Parkinson, Dr O'Brien, Dr Duncan Junior, and the present author, it may be doubted whether their nature and origin is very clearly explained. It was the opinion of Bonetus, that, in cases of discharge from the ear, the purulent matter indicated an abscess in the brain from which he supposed it to proceed. Though this notion was satisfactorily refuted by Morgagni, it was revived by Mr Brodie, who seems to think the effusion of the brain coeval at least with that of the ear. Knowledge of the anatomical relations of the internal ear, nevertheless, is all that is requisite to explain the origin and cause of the disease. In all cases almost it originates in inflammation of the fibro-mucous membrane which lines the tympanal cavity, proceeds backwards to the mastoid cells, and is continued inwards and forwards through the *fenestra ovalis* and *fenestra rotunda* into the vestibule and *cochlea* respectively. In following this direction it is easy to see with what facility inflammatory action is propagated to the *dura mater* investing the petrous portion of the temporal bone, and especially that part of it which descends into the internal auditory hole on the posterior surface of the pyramid. Here indeed the *dura mater* is almost in direct contact with the fibro-mucous membrane of the vestibule. It hence results that inflammation occurring in the tympanal cavity, and assuming, as it often does, a mild and chronic character, under the shape of a mere ear-discharge (*otorrhoea*,) is too often neglected until it spreads either into the mastoid cells, or through the vestibule and *cochlea* to the meningeal covering of the temporal bone, when it sooner or later, sometimes very speedily, causes extensive detachment, suppuration, and ulceration of the membranes, and even abscess in the adjoining surface of the brain or cerebellum. These effects, which were originally observed by Morgagni and latterly by Powell, are well illustrated by the cases of the work before us.

Though it is most common to see the disease confined in this manner to the surface of the brain, in the cases of Bianchi, Mr Brodie, Mr Parkinson, Dr O'Brien, and in one of those of Dr Abercrombie, suppuration took place in the interior substance of the cerebellum. Under such circumstances it not unfrequently happens that the discharge, after continuing for months or even years at various rates, suddenly ceases, or even is suppressed after surgical treatment; and this suppression marks the commencement of the acute meningeal or cerebral inflammation which terminates life.

The connection between disease of the brain and discharge from the nose, that is, chronic inflammation of the fibro-mucous membrane lining the ethmoid plates and cells, or the frontal sinus,—a connection adverted to by Dr Abercrombie, after Bonetus, Morgagni, and Lieutaud, is to be viewed as part of the same general anatomico-pathological fact. In short, the intimate and almost direct connection which exists between the fibro-mucous lining of the horizontal perforated plate of the ethmoid bone, and the *dura mater*, which covers its inner surface, is sufficient to show the easy transition of morbid action from the former to the latter.

With morbid states of the *dura mater* Dr Abercrombie arranges certain changes incident to the venous canals or sinuses interposed between its plates. Though these changes belong perhaps to those of the venous system in general, yet it must be conceded to the author that their occurrence depends much on disease of the *dura mater* or contiguous bone. This is well illustrated in the fifth case, in which the left lateral sinus contained purulent cheesy matter, without blood, and with thickening of its coats to such a degree that its cavity was at one part nearly obliterated. The jugular fossa was carious for the space of a shilling; and the pyramidal portion of the temporal bone was extensively carious. Does such an unsound state of the sinuses ever occur independent of disease of the bone?

Under the general appellation of *Meningitis* are placed inflammation of the arachnoid membrane and of the *pia mater*, which Dr Abercrombie justly remarks it is difficult to distinguish either in practice or pathologically. So intimate, indeed, is the anatomical connection of these membranes, that the circulation of the one almost constantly influences that of the other; and one of the most usual effects of distension of the vessels of the *pia mater* is effusion of serous fluid in the subarachnoid cellular tissue, or from the arachnoid itself, or of albuminous exudation from its surface. This is well known; and the principal modifications of disease occurring in different regions of the exterior meninx are well illustrated by cases in the present work. One important circumstance, however, which indeed is by no means generally understood, is not adverted to either under the present head or in any other part of the volume; for neither the first nor the fourth subdivisions of the sixth section can be considered as belonging to this head. We allude to the *meningitis* of the internal or central portion of the membrane. The choroid *plexus*, though known to be of a similar nature to that of the *pia mater*, is, however, seldom regarded as a direct continuation of it, possessing similar properties, performing the same function, and liable to the same morbid states. The choroid plexus also is not confined to the lateral ventricles, and extends not only into the inferior and posterior *cornu*, but into the fourth ventricle, at the bottom and side of which its connection with the *pia mater* may again be traced. Inflammation is perhaps more common in this division of the cerebral membrane than in its outer part. It is this inflammation which is the cause both of the symptoms of *hydrocephalus*, and of the watery effusion occasionally found after death; and it is a circumstance not unimportant in a pathological light, that the inflammatory process taking place in this membrane is attended with preternatural dryness of the exterior division,—indicating an alternation of action between the two. It is the same congestion and effusion which takes place in tedious cases of fever and various febrile diseases, with symptoms of affection of the brain.

Inflammation of the substance of the hemispheres, or rather of that of the brain, is illustrated at great length under the heads of vascular injection, pulpy destruction, *(ramollissement,)* either alone or with purulent matter, undefined suppuration, encysted abscess, and ulceration of the surface of the brain. The three first of these at least are different degrees only of the same morbid process.

In the first case, which is also the first stage, a portion more or less extensive of the substance generally of the hemisphere becomes of a red colour, highly vascular, and in a slight degree less consistent than the surrounding brain. In the second case this state of vascular injection may be traced passing gradually into that of softening, when part of the substance becomes soft, yellowish or ash-coloured, much disorganized, and either with or without spots or patches of purulent matter. When the latter is found either in an irregular ragged cavity, or in a cluster of communicating cells, it constitutes a fourth form of the disease, being one variety of cerebral abscess; and when it is inclosed in a membranous cyst more or less consistent, it constitutes another, as the encysted abscess.

From the observations of Bonetus, Morgagni, Lieutaud, Baader,[*] Baillie, Powell, Brodie, Hooper, and the present author, no doubt can be entertained that, with the exception of the last, the whole of these morbid states are to be viewed as different stages of the same process. Dr Abercrombie, ascribing these varieties to different degrees of activity in the inflammatory process, is inclined to the opinion, that in the most active form of the disease it advances speedily to ramollissement or suppuration; and that in more chronic forms, in which it proceeds slowly, it may produce urgent symptoms for some time without advancing to an irrecoverable state. After this he thinks there is reason to believe it may pass into suppuration or induration of the cerebral substance, and thus be a cause of organic change.

That the pulpy destruction is an effect of inflammation we formerly attempted to show, when considering the researches of MM. Rostan and Lallemand. Dr Abercrombie, who had occasion to take the same view of the subject in his original essays, adheres to it, but regards the change as analogous to gangrene in soft parts. This idea is by no means new; for not only was it that of Bonetus, Morgagni, and Lieutaud, but more recently of Jemina and Baillie. Indeed Bonetus, who has repeatedly mentioned the softened state of parts of the brain, seemed to think that the only way of accounting for the preternatural diminution of consistence was by ascribing it to mortification; and the same idea is currently repeated by many authors and collectors to the present time. Jemina particularly, who saw this change in the bodies of persons cut off by a febrile epidemic at Montreal in the Turin territory in 1783–84,[*]

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[*] Josephi Baader, Observat. Med. Obs. 32. Ext. apud Sandefort Thesaurum, Vol. iii. p. 28.

[†] De Febre, anno 1783–84. Monteregali Epidemica, Auctore Marco Antonio Jemina, M. D. &c. Extat in Brera Sylloge, Vol. x. p. 218. 247.

after describing it, asks, *annon gangrena hujus visceris?* and the same appears to have been the idea entertained by Dr Baillie.

No doubt can be entertained, we conceive, that it is the effect of inflammation, acute or chronic; for it takes place not only as a consequence of this occurring spontaneously, but also from mechanical injury inflicted on the head, and even as a consequence of the vascular distension which attends tumours, morbid induration, and other organic changes of the brain. That it takes place in consequence of injury, appears not only from the cases recorded by Morgagni, Quesnay, Morand, but from those given by Dease, Schmucker, Abernethy, Dr Thomson, and Dr Hennen. This fact is particularly illustrated by the cases of Schmucker, in which it is quite common, after injury of the head, to find more or less of the brain reported soft, pulpy, and dissolved. * That the same change may take place in consequence of the presence of tumours, may be shown from a good case given by Meckel, from one by Dr Yellowly, from two of Dr Powell's cases, and from others in the works of Bonetus and Lieutaud. In several cases noticed by Morgagni it appears to have existed for a considerable time. (Epist. ix.) From these circumstances, it may be doubted whether the opinion of the present ingenious author, regarding the resemblance between pulpy destruction of the brain and gangrene, is in all cases applicable. So far as the part of the brain is disorganized it may be said to be dead; and regarding it in this sense as gangrene, it would not be easy to prove, that, as a consequence of acute inflammation, it was not so. But when it is of long duration, as is the case in tumours and other organic changes, it is difficult to suppose that part of the brain is dead during any time of the life of the individual. On these objections, however, which we state as difficulties occurring in our apprehension to this view, lest we give them too much weight, we dwell no longer.

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* "Die darunter liegende *substantia corticalis* des Gehirns war aufgelosst (dissolved); und gab einen faulen Geruch von sich."—"Bemerkte ich, dass ein Theil des Gehirns sehr verandert war, und bey der Beruhrung ward ich eine bewegliche Materire gewahr. Ich ofnete diesen Ort, und fand eine hoele, in welcher, uber ein Loth dickes zahes geronnenes Blut enthatten war; unter demselben war die *substantia medullaris* des Gehirns in eine gelbe weiche Feuchtigkeit (ohngefehr von der Art, wie sie in einem *meliceride* gefunden wird) verwandelt. Die ganze innere Flaeche der Hoele, war verfault, und in eine braune Gauche aufgelösst." Chirurgische Wahrnehmungen, Von Johann Leberecht Schmucker, Konigl, &c. Erster Theil, Berlin, 1774.—"The whole right hemisphere seemed to be reduced into a pulpy and fetid mass, composed of a mixture of blood and brain." Abernethy, case xi. p. 51.—"We found the right hemisphere of the cerebral mass reduced to a sort of bloody pulp, still retaining some shape of what it had been, but much diminished in size, and rendering it absolutely impossible to trace its organization." Hennen, p. 43.—"He died at the end of six weeks; and the whole hemisphere of the brain on the side of the injury was found converted into a soft reddish substance." Report of Observations by John Thomson, M. D., &c. Edin. 1816. p. 55.—"On the death of the patient nearly the whole of the right hemisphere was found converted into a soft pulpy mass." Ibid. p. 59.

On the semeiography of this morbid change we were much pleased to find Dr Abercrombie differing entirely from the French writers who have treated this subject. By most of these pulpy destruction has been represented as attended with and distinguished by tonic spasms and twitches of the muscles of the extremities. From cases which have fallen under our own observation, as well as from those which we see recorded by Morgagni, this inference appeared to us precipitate, and derived from partial observation only. Dr Abercrombie thinks there is no foundation for it, and remarks that the change in question seems to be distinguished by no uniformity of symptoms. It is indeed to be observed, that no morbid change is found connected with such varied, and sometimes opposite assemblages of symptoms. It is worthy of remark, that in many instances, particularly in the chronic form of the disease, it causes little or no sensible derangement in the muscular motions, and is on the contrary sometimes the cause of fatuity, idiotism, or mental imbecility. This is especially remarked in that form of mental imbecility which succeds the *coup de soleil* of tropical countries. In other cases, again, we have seen it connected with hemiplegia and impaired memory; and in several instances it is the only morbid change found in the brain of epileptic subjects, in whom there were distinct intervals of health. Morgagni himself states, that it was found in the gray matter of the brain of Marchetti, the anatomist, who, after two epileptic attacks, died apoplectic. (Epist. vii. 14, 15.) Dr Abercrombie represents them to be attended by convulsion more or less extensive, followed by palsy and coma; but admits that in some instances others are destructive of the cerebral substance, without either palsy on convulsion, and even without coma. It is by no means improbable that subsequent observation may show that this want of uniformity, and the varied character of the symptoms, depends in a great degree upon the part of the organ which is disorganized. It is also to be kept in mind that many of the symptoms depend not so much upon the destruction of the part as on the vascular irritation of the contiguous cerebral substance, resulting from the morbid injection which terminates in the change.

The various forms in which the termination by suppuration takes place may be referred to three; 1*st*, an irregular cavity, such as that delineated by Baillie and Hooper in the substance of the hemispheres; 2*d*, a small abscess in some part either of the anterior or the posterior lobe; and, 3*d*, an abscess of various size, but enclosed in a firm cyst. The two former are doubtless the result of acute inflammation. The latter is perhaps in most cases the result of a chronic form of the process. Another variety of puriform cavity is that which is occasionally the result of a broken down or suppurated tubercular mass,—a good example of which is given in the work of Dr Monro, noticed in last number.

From the researches of the present author, it appears that the formation of these collections is attended with not less diversity of symptoms than that of pulpy destruction. It has been indeed frequently remarked by various authors, that purulent collections take place in various parts of the brain, without ever giving rise to conspicuous symptoms; while, conversely, very severe symptoms have risen from very slight and partial effusion of matter. In the elaborate paper of M. la Peyronie may be found examples of suppuration, either spontaneous or from injury, in almost every part of the brain, without giving rise to any symptoms which were sufficiently urgent to attract notice, till the last hours of life, when life was suddenly terminated by convulsions or coma. The writings of surgeons abound with similar cases, from Morand and Quesnay down to Abernethy and Sir Everard Home.* Dr Abercrombie inclines to the inference, that in the encysted abscess these symptoms are more distinct and severe; and he adduces a variety of instances from Morgagni, Lallemand, Broussais, Bateman, and Powell, to show, that though in these cases various derangements of sensation and locomotion take place, yet it is impossible to trace any uniform connection between the extent of the lesion and the external symptoms. Much the same may be predicated of those examples of abscess of the cerebellum which have been recorded by Bianchi, Janus Plancus, Stoll, Douglas, Weikard, and the author of the present work.

Ulceration of the surface of the brain, which is properly regarded as rare, has not been seen by Dr Abercrombie. So far as we are aware, it was first distinctly noticed by Ridley, who saw it destroy the outer gray matter of the convoluted surface without pain or affection of the senses. † Dr Anderson of Leith saw it after long-continued pain of the crown of the head; ‡ and Powell in like manner met with superficial ulceration of the anterior part of the right hemisphere. § Most frequently, however, it has been seen in the foliated surface of the cerebellum. Thus Haller mentions an eminent seal-engraver who perished from an ulcer of the cerebellum after slow loss of the senses and fatuity.[*] Maximilian Stoll found an instance of it in the cerebellum of a young man of 20, accompanied with redness, thickening, and erosion of the *pia mater* ;[†] and two cases of the same character are recently recorded by Scoutetten.[‡] From the circumstances of the necroscopic appearances in these cases, it may be stated as a general result, that ulceration of the convoluted surface of the brain or the laminated surface of the cerebellum is an effect of circumscribed inflammation of the *pia mater*.

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* Transactions of a Society, &c. Vol. iii. p. 94, and Observations on the Functions of the Brain, by Sir Ev. Home, Bart. V. P. R. S. Phil. Trans. 1814, p. 469.

† Observationes quædam Medico Practicæ, &c. Auctore H. Ridley, M. D. &c. Lugduni Bat. 1750. Obs. 34, p. 212.

‡ Transactions of Royal Society of Edinburgh, Vol. ii.

§ Transactions of College of Physicians, Vol. v. Case 6.

Chronic inflammation of the cerebral substance, which forms the last subject of examination in this section, may prove fatal in the inflammatory stage without suppuration, or it may cause induration of part of the brain, which may then be dissolved in unhealthy suppuration. Induration of the brain has been noticed by Morgagni, and particularly dwelt on by Meckel as a cause of mental derangement in general, and fatuity or imbecility in particular. The latter author, in a selection of interesting cases described in the Memoirs of the Academy of Berlin, found the substance of the organ not only firmer and harder than natural, but, though more elastic, actually diminished in specific gravity, a cube of six lines diameter being upon the average two grains lighter than a similar cube taken from a sound brain. From his incidental remarks, it is manifest that Meckel ascribed this change to a degree of compression or condensation resulting from the vessels being inordinately loaded ; and it is further manifest from the necroscopic reports, that the cerebral vessels were in a state analogous to inflammation. (Obs. 7, 8, 9, 10.) He, however, allowed his observations to be biassed by hypothetical notions on the animal spirits and mechanical views of their compression. Lately M. Serres has advanced an opinion much the same as that which is mentioned by the present author, viz. that the vascular injection which constitutes apoplexy not unfrequently terminates in chronic inflammatory action and induration of the brain. The view which Dr Abercrombie gives of its nature is ingenious, and, so far as we can perceive, just. He regards it as the result of low scrofulous inflammation, that is, we presume, chronic vascular congestion occurring in subjects termed scrofulous ; and that, though in the early stage it may be under the control of remedial means, the formation of a cyst of coagulable lymph, or its gradual induration, give it the character of organic and unmanageable disease. The natural termination of this change, unless the patient is cut off by it early, is represented to be suppurative softening or destruction of the part, or, if near the surface, of the brain, the form of ulceration already mentioned.

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[*] Elementa Physiologiæ, Vol. iv. lib. x. sect. vii. § xxxvi. p. 351.

[†] Ratio Medendi, pars 3, p. 122. [‡] Archives Generales, Tom. vii. p. 31.

Inflammation of the central parts, by which are meant the *fornix*, *septum lucidum*, and *corpus callosum*, is illustrated at considerable length, and with much accuracy. According to Dr Abercrombie it occurs under two different forms ; inflammation of the membrane lining the ventricles, by which we imagine is meant the epithelion of these cavities ; and inflammation of the white cerebral substance of the *corpus callosum*, *fornix*, and *septum lucidum*. It may be doubted whether this distinction holds good ; for the difference appears to consist simply in the inflammatory action spreading, in the one case, along the surface, and in the other, sinking into the substance of these parts. Again, it may be doubted whether the inflammatory action of the surface is in any case confined to that without affecting the parts, or conversely, whether, when seated in the substance, it does not affect the surface. Thus, when the *septum* is softened and perforated, or even broken down, which Dr Abercrombie justly represents as one of the effects most frequently characteristic of the second form of inflammation, we invariably find in the ventricle fluid, sometimes limpid, sometimes turbid, which the author also regards as the product characteristic of the former affection. The same may be said of destruction of the fornix, which is never, so far as we have seen, unaccompanied with more or less fluid in the ventricles. Conversely, when fluid is effused into the ventricles, the septum is very frequently, if not always, reticular, perforated, or actually broken down. The only exception to this is, that fluid may be found in the ventricles without much injury of the fornix, and with none of the mesolobe. It must nevertheless be admitted, that the author adduces cases of effusion into the ventricles without destruction of the central parts, and destruction of the central parts without effusion. To counterpoise this, however, he admits that the combination of the two is the most frequent form of the disease named acute *hydrocephalus*, and of this the greater part of his cases are instructive examples. The four last cases which he adduces as instances of simple effusion we are disposed to regard as the effects or products of inflammation of the choroid plexus, which indeed, from what has fallen under our observation, is the most frequent source of watery effusion into the cerebral cavities, especially when that effusion is unaccompanied with pulpy destruction of any of the contiguous parts of the cerebral substance.

In investigating the nature and effects of this preternatural effusion of fluid, Dr Abercrombie directs the attention of the reader to a pathological question of the utmost practical importance. Many cases recorded by a variety of authors of very competent observation and unquestionable veracity, show that serous fluid may be effused into the ventricles of the human brain without producing any very marked symptoms either in sensation or locomotion, and certain with none in the actions of involuntary motion. Conversely, in not a few cases in which all the symptoms said to denote oppression and compression of the brain have been present, not a particle of fluid has been found; or, if any was present, its quantity bore no proportion whatever to the intensity of the symptoms which during life it was imagined to have caused. The natural inference from these facts, the accuracy of which all persons who have had any experience in observing disease and tracing the state of the organs in the dead body will verify, is, that the convulsions, coma, and other forms of deranged sensation and motion, are the effect not of the effusion, but of the morbid process, which terminates in the effusion; that the presence or the degree of effusion is indicated by no uniform symptom or symptoms; that death itself is in all probability more frequently the result of this previous morbid process; and that these symptoms are connected with a state of the meningeal capillaries, doubtless inflammatory, which, if attacked with prompt and energetic measures, it is within the power of the physician to control and subdue.

Upon the knowledge of the facts and inferences now adduced, the only question is, whether hydrocephalus has been or may be cured? and Dr Abercrombie properly enough resolves it, by saying, that cases which present all the symptoms known to indicate the disease, we presume in its inflammatory state, have recovered under the use of proper remedies. As to the removal by absorption of the fluid when actually effused, which the learned author seems disposed to believe may take place, we fear no satisfactory proof of the fact can be adduced; and it may be doubted whether, considering the destruction of the contiguous parts, it would avail. It is manifestly not, as mere effused fluid, the cause of death; and the fatal event is determined according to the properties and laws of the animal textures before this effusion has taken place.

The ordinary causes, and the most successful method of treatment, are well illustrated in the seventh and eighth sections, with which the first part of the work is concluded.

We have already adverted to the defects of the mode of division adopted by the author of the present work; and of this the appendix to the part now terminated is an unfortunate example. It is possible, certainly, that the second and third sections, on affections of the cranium and pericranium, may be properly enough referred to this situation. But, with deference to the judgment of the author, we cannot refrain from thinking that the first section, the subject of which is tubercular disease of the brain, would have occupied a much more suitable station in the third part, among organic changes. It is indeed scarcely possible to imagine a more complete example of organic disease than tubercular disorganization. It is true, as the learned author states, that tubercular growths are allied to inflammation, both in the changes which they induce in the circulation of the adjoining texture and in the symptoms. But of what organic change may the same quality not be predicated at some stage of their progress? These remarks are submitted with that respect for the judgment of the author to which his accurate habits of investigating the nature of disease show that he is justly entitled. The cases adduced are excellent and instructive illustrations of the general and particular characters of tubercular deposition of the brain, and should be minutely studied by those who wish to be familiar with this modification of organic disease. If we have occasion to refer to them, it shall be done under that head.

The peculiar destruction of the inner table of the cranium, mentioned in the second section of the appendix, is doubtless singular, in so far as the morbid action was not confined to one spot, but was diffused over the whole inner surface. A case somewhat similar, but not advanced to the same degree, we have seen. In this the inner table was in the progress only of absorption, being removed to some depth over a very great extent of the scull, and with portions of the original bone left above the absorbed surface. In this case the agents of absorption were the vessels of the outer or cranial surface of the *dura mater*. The man had received a blow on the head some months before. Much the same sort of action appears to have taken place in the case recorded by Dr Robert Evans, in the present number, (p. 288.)

Dr Abercrombie justly opposes the doctrine inculcated by some surgical authors, that in cases in which the bone becomes diseased after wounds and injuries of the head, it is caused by detachment of the *pericranium* or *dura mater*. It is indeed by no means uncommon for the former membrane to be completely detached from the scull, yet without causing either death or caries of the bone. We have seen even a crop of healthy granulations arise from the exposed surface under those circumstances; and the same, doubtless, must have occurred to others. The inflammatory action consequent on the injury is justly regarded by the author as the probable cause of caries or death of the bone. It may be further said that this result is most likely to take place when the injury is so severe, or the consequent inflammation so general, as to affect at once the *pericra-*

nium and the *dura mater.* To speak of the affections proper to the former membrane is here superfluous.

The second general division of the work on *apoplectic affections* is introduced with some general observations on the manner in which the apoplectic seizure takes place. The three heads to which the author refers these several forms of invasion might perhaps have been rendered more clearly intelligible had they been connected with the pathological states of the organ on which they depend. But as the author must have seen reason for avoiding this, either in its inexpediency or its impracticability, it is unnecessary to urge the matter further.

One of the points in the pathology of apoplexy which has most puzzled physicians, and given rise to so many discordant statements and so much difference of opinion regarding the treatment to be pursued, is that of cases to all appearance completely apoplectic, that is, with loss of sense and motion, terminating fatally, yet without extravasation of blood or any of those appearances which, since the time of Wepfer, have been conceived necessary to produce apoplexy. The undeniable occurrence of examples of this description furnished ground for the establishment of a form of the disease called *nervous apoplexy*,—a distinction which was in 1780 formally admitted by Zuliani of Brescia, and Kortum of Dortmund in 1785, and which was some time after recognized in this country by Kirkland, and more recently by Abernethy. In this it was imagined that apoplectic symptoms might occur, independent of any lesion of the brain or its membranes appreciable by dissection, and purely in consequence of a torpid condition of the organ, or a suspension of its nervous energy, generally to be traced to disorder of the gastro-enteric organs. *

The present author, without adopting the views of those now mentioned, admits, however, a similar form of the disease, to which, as taking place *without any morbid appearance*, he applies the appellation of *simple apoplexy ;* and in confirmation of which he relates four cases of apoplectic death. In the first and fourth of these, indeed, it is stated no anormal appearance was recognized. In the second, the meningeal vessels were inordinately distended ; and in the third, not only were the veins turgid, but the cerebral vessels were highly injected. That the appearances in the latter were adequate to account for the fatal event, we shall presently attempt to show. The former we confess our inability to explain, unless upon the same principle on which, in the bodies of persons cut off by inflammatory symptoms, the absence of distinct traces of the morbid action is accounted for. The morbid action in such circumstances, considered as a living process which had not advanced far enough to generate morbid products, has been supposed to disappear with life;—an argument which deserves some support from the fact, that, even where the morbid products are distinct, the change in organization which had led to it is occasionally very obscure, or even almost equivocal.

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* F. Zuliani de Apoplexiâ præsertim Nerveâ. Lipsiæ, 1780 ; and Car. G. Theod. Kortum de Apoplexia Nervosâ. Gottingæ, 1785, Apud Ludwig, Tom. iv.

Without entering minutely into the merits of this distinction, for the proofs of which we refer to Zuliani and Kortum, we think there are good grounds for believing that it rests on a mistaken notion of the pathological cause of apoplexy. Wepfer, the first accurate writer on the morbid anatomy of the disease, laboured to establish the general inference, that extravasation of blood was necessary to constitute apoplexy; * and to this idea, which was further extended and confirmed by the researches of Bonetus, Hoffmann, Valsalva, Morgagni, and others, may be referred much of the misconception which has prevailed on this subject. When the idea of the necessity of actual extravasation had once occupied the mind, it was difficult, if not impossible, to explain cases of apoplectic death which took place without extravasation.

That this notion of the necessity of extravasation is gratuitous may be inferred from the following facts and considerations. In the first place, it is certain that complete apoplexy may take place from mere injection only, either of the meningeal or of the cerebral blood-vessels. Thus not only Morgagni, but even Wepfer and Valsalva, strong as their predilection for the necessity of extravasation was, met with cases in which none had taken place. The physicians of Breslau also in their reports, and Quarin in his commentaries on them, remarked as an extraordinary fact, that apoplectic death might occur without extravasation. The possibility of this was admitted by Cullen and Rochoux, and necroscopically demonstrated by Baillie. "The milder forms of apoplexy," says this physician in his posthumous observations, "depend upon a distension of some of the vessels of the brain, from undue accumulation of blood in them. I have known, however, *one instance of fatal apoplexy* where many of the blood-vessels were found, upon examination after death, to be much distended with blood; but no blood had been extravasated in any part of the brain." † Secondly, the experiments of Serres on living animals show that, conversely, extravasation of blood may take place to a considerable extent without inducing apoplectic symptoms. * Thirdly, it has been shown by the same physician, and must have been observed by many besides, that the apoplectic symptoms disappear while the extravasated blood remains. Fourthly, the apoplectic symptoms bear no relation or proportion to the quantity of blood effused;—complete and distinct apoplexy being often associated with the extravasation of a minute quantity, while copious extravasation is not unfrequently connected with very mild and temporary apoplectic symptoms.

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* Joh. Jacobi Wepferi Historiæ Apoplecticorum observationibus, &c. illustratæ. Amstelædami, 1724, pp. 210, 211, &c.

† Lectures and Observations on Medicine. London, 1825. p. 167

From these facts it is to be inferred, that extravasation of blood is not necessary to constitute apoplexy; and that the essential anatomical character of this disease is injection and distension either of the meningeal or of the cerebral vessels, or of both. It is manifest that in all cases of extravasation, except those from rupture of a large trunk, this *must* be present; whereas its termination in extravasation is merely a probable effect of the previous injection. It may be further demonstrated, that, in the cases of serous effusion with apoplectic symptoms, commonly named serous apoplexy, the true pathological cause is not the effusion, but the vascular meningeal injection from which effusion arises. This doctrine, which was obscurely conjectured by Morgagni, was distinctly stated by Stoll,† fully established by Portal, and newly confirmed by M. Serres and the present author.

It must, nevertheless, be regarded as a defect in the general pathological views of Dr Abercrombie, that, although perfectly aware of the influence of vascular injection in producing serous effusion, so far as we can discover, he no where states distinctly the doctrine, that vascular injection alone is adequate to produce, and actually does produce, apoplectic symptoms. We are perfectly aware, that he no doubt felt himself bound to admit the force not only of his own cases, but also of those of Dr Stark and Dr Powell. Of the four cases of Dr Abercrombie we have stated our opinion above. That of Dr Stark we have always regarded as an example of apoplectic symptoms, depending on vascular injection of the *medulla oblongata* and spinal chord; for though the ingenious author himself admits that the chord was not examined, he states, " that about an ounce or more of bloody fluid ran out from the spinal canal;" ‡ a circumstance, it may be remarked, which does not take place unless where the vessels are much loaded. The case of Dr Powell is certainly more perplexing; and, like the first and fourth of Dr Abercrombie, we shall not attempt to explain it. But to all these cases the general objection applies,—that as the dissectors, under the impression of the doctrine of Wepfer, always looked for extravasated blood, when they failed to find this, they therefore said that there was no morbid appearance. It is manifest, that, as what is morbid appearance must be determined in relation to what is sound, it is not easy, in any given case, to draw the line of distinction with positive certainty.

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* Nouvelle Division des Apoplexies, par M. Serres; Annuaire Medico-Chirurgicale des Hopitaux et Hospices Civiles de Paris. A Paris, 1817. 4to. pp. 241, 260, 261.

† Ratio Medendi, Pars I. p. 138.

‡ Works of the late William Stark, M. D., &c. Part iv. sect. iv. p. 79.

But even after the necessary allowances for this source of fallacy, another, by no means either unimportant or imaginary, demands consideration. We allude to that class of cases which have been assembled by M. Serres, under the head of *meningeal apoplexy without effusion.* We are not unaware that some physicians overlook the distinctions of this ingenious pathologist, or hear them mentioned with an incredulous sneer, as the imaginary creations of a wild theorist. To such these remarks are not addressed; nor is it requisite to reason where the ground is preoccupied by prejudice. To those, on the contrary, who are open to conviction, it may be necessary to say, that M. Serres shows distinctly that apoplectic symptoms may, and often do arise from mere vascular injection of the proper cerebral membrane, without effusion, either serous or sanguinolent. Of this description are his 16th, 17th, and 18th cases, in which the vessels of the *pia mater* were more or less injected without other morbid change. Similar cases must have occurred to several practical observers; and we submit to the ingenious author of the present work whether the 2d and 3d of his cases may not be referred to this head.

For these reasons, it may, without the imputation of trivial objection, be justly questioned, whether there is ground for the nervous apoplexy of Zuliani, Kortum, and Kirkland, or by parity of reason, for the simple apoplexy of the present author. As the matter at present stands, it is wisest to adopt the side which does not recognize this form of apoplectic disease.

On this subject we have found it our duty to dwell at so great length, that our observations on the others must be brief. The first, indeed, that of apoplexy with bloody extravasation, will not require particular remark. The cases and observations of Dr Abercrombie illustrate the old doctrine of Wepfer and Morgagni. One or two points only may be brought forward.

The sources of hemorrhage are by Dr Abercrombie referred to the following heads:—1*st*, The deap-seated or the cerebral vessels; 2*d*, the superficial or the meningeal vessels; 3*d*, ulceration or rupture of an arterial trunk; 4*th*, the vessels of the choroid plexus; 5*th*, rupture of a sinus; 6*th*, rupture of small aneurisms in various parts of the cerebral vessels; and, 7*th*, from erosion of a vessel between the *dura mater* and the bone, in consequence of caries of the latter. Pathologically this division errs by its multiplicity; and it is further not a little confused. Hemorrhage from the choroid plexus belongs to the same general head with that from the meningeal vessels; and rupture of the aneurismal sac of a cerebral vessel cannot be so different from that of ulceration or rupture of an arterial trunk as to require a separate head. Of the sources of hemorrhage within the head, a more natural arrangement would be the following. I. Those hemorrhages which depend on previous capillary injection, and take place from numerous minute arteries; the *active hemorrhages* of scholastic writers. Of these there are two sorts, 1*st*, hemorrhage from injection of the cerebral vessels; and, 2*d*, hemorrhage from injection of the meningeal vessels. II. The hemorrhages resulting from rupture, laceration, or erosion; 1*st*, of an artery, either aneurismal or simply diseased; 2*d*, from a vein or sinus, whether by disease or by laceration;—the passive hemorrhages of authors.

Dr Abercrombie very properly shows, after Dr Cheyne, that it is impossible to trace the extravasation to particular vessels, and that many capillaries are found to open into the cavity in which the blood is effused. When it takes place from the deep capillaries of the substance of the brain, which is most frequent, it constitutes the true *cerebral hemorrhage* of Hoffmann, the *cerebral apoplexy* of Serres.

The second form mentioned by the author, which must also be regarded as an active hemorrhage, is that which results from injection of the capillaries of the proper cerebral membrane,—the *pia mater* and choroid plexus. It presents two subdivisions, according as the hemorrhage occurs on the convoluted, or on the central surface of the organ,—from the *pia mater*, or the choroid plexus. This corresponds with the meningeal apoplexy, *avec epanchement sanguinolent*, of M. Serres, (§ vi. Cases 20th, 21st, and 22d, from Wepfer.)

Under the first subdivision, or that of hemorrhage from the *pia mater*, in addition to the cases communicated to the author by Dr Hunter and Dr Barlow, we may be allowed to add, that a most distinct and instructive example of the disease is given by Mr Howship in his 14th case. It occurred in a young woman of 22, who for two years had suffered much from rheumatic ailments, and had at length, after paralytic and vertiginous symptoms, died lethargic. Upon inspection, the *pia mater* was found vascular and red; its vessels increased in number and size; and blood was diffusely extravasated all under the *pia mater*. "The extravasated fluid had formed superficial *coagula*, corresponding to the *sulci* between the convolutions of the brain."—"It had taken place very universally, and the effusion seemed to have arisen not only from the capillary arteries upon the external surface of the *pia mater*, but also from those processes of the membrane which dip down between the convolutions forming the *tomentum cerebri.* Several of these deeper seated *coagula* were divided by the knife in the course of the dissection." *

To the second subdivision, or hemorrhage from the choroid plexus, are to be referred those instances of sanguinolent or bloody extravasation which take place in the ventricles without rupture of their *parietes.* In ordinary cases of cerebral hemorrhage it is not uncommon to find in the ventricle masses of clotted blood, which may always be traced to laceration of the roof, the side, or the floor, by blood issuing from an apoplectic cell in the adjoining cerebral substance. In this, however, the fluid is rather sanguinolent than pure blood ; and there is no laceration of the cerebral substance which forms the floor or the wall of the ventricle. As in the serous membranes, it is a hemorrhage by exhalation. In the case of De Haen, mentioned by almost all authors who have written on this subject, the fluid, which was blood, escaped by rupture of the vessels of the plexus.

On the passive hemorrhages it is unnecessary to dwell. We formerly took occasion, when speaking of the researches of MM. Bouillaud and Serres, to examine somewhat at length this source of hemorrhage, as occurring from morbid arterial trunks. It is manifest that, in pathological nature, it differs much from the genuine cerebral hemorrhage of apoplexy ; and this difference constitutes a most material practical one. Extravasation from rupture of the lateral sinus we have seen once only occasioned by external injury of the head.

The influence of the morbid state of the arterial tissues of the brain, which has occupied the attention of Morgagni, Vater, Scarpa, Blane, Baillie, Hodgson, Howship, Bouillaud, and Serres, receives also a portion of attention from Dr Abercrombie. Though he does not so positively as Bouillaud argue for arterial rupture arising immediately from disease of the tunics, he admits that this morbid change may alter the circulation so much, as to induce serious complaints within the head. From the cases recorded we think it a legitimate inference to say, that it often produces vascular injection, which may terminate in serous effusion or bloody extravasation, according to circumstances. This is clearly established by the facts formerly adduced, and more especially by the dissections of Mr Howship, which, next to one or two by Hodgson, are perhaps the clearest and most pointed on record. (Cases 17, 18, 19, 20.) In each of these cases, though the arterial trunks of the brain were extensively and seriously diseased, the cerebral hemorrhage had taken place, not from the diseased trunks, but from the capillaries in the substance of the brain.

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* Practical Observations on Surgery and Morbid Anatomy, illustrated by Cases, with Dissections and Engravings. By John Howship, Member of the Royal College, &c. London, 1816. Section ii. Case 14.

This derives further confirmation from what is stated by Dr Abercrombie in the second division of this subject, to which we now proceed,—paralysis with serous effusion, bloody extravasation of limited extent, or pulpy destruction. Each of these states he shows to be generally connected with an unsound condition of the cerebral arteries, often affecting them to a very considerable extent.

Of palsy in general, the natural history and pathology are delivered at great length and with much accuracy; and the enumeration of the pathological states of the brain and its membranes, with which the several degrees and forms of this disease are connected, is evidently derived with much care from an extensive collection of facts. From all these the general result may be stated to be, that, though palsy is an effect of any change in the organization or structure of the central nervous mass, many of these changes may exist without producing it. It is probable, therefore, that the pathologist has still to look for some general pathological circumstance which is common to the whole of these, but the operation of which may be prevented from taking place by circumstances with which we are yet unacquainted. This, it must be admitted, is general, if not vague and unsatisfactory. But it does not appear from the researches of the present author, that it is possible to make a nearer approach to the precise and universal pathological cause of this malady.

The appendix to part second contains some ingenious conjectures regarding the circulation of the brain, and the changes which it is liable to undergo. From the experiments and observations of Dr Kellie, with other considerations derived from the anatomical peculiarities of the parts within the scull, the author regards it as highly probable, that, in the ordinary state of the parts, no material change can take place in the absolute quantity of blood circulating in the vessels of the brain. The blood moving in these vessels, nevertheless, he thinks, being divided in a certain ratio between the arteries and veins, may be liable to sundry alterations in this ratio; and upon the accurate adjustment of the circulation in these two systems, he conceives the healthy state of the organ may mainly depend. In his further observations on this subject, he attempts to trace generally the several modes in which, in an organ circumstanced as the brain, derangement of the circulation may be supposed to take place.

The first of these is from the general plethoric state of the person, in which the vascular system at large is understood to be distended with an inordinate quantity of blood, and in which, consequently, the cerebral vessels may be supposed to partake of the general injection.

The second cause of this nature Dr Abercrombie thinks may be found in any circumstance which may interrupt the return of the blood from the veins of the brain. Little doubt can be entertained that on certain occasions this cause may operate; and the author is perhaps not much mistaken when he suggests the possibility of its taking place in that form of the disease which he names simple apoplexy. It is nevertheless so seldom that the actual existence of the cause can be recognized, that we think it impossible to assign it more than a partial and a probable influence.

To the third cause of this description, diminution of capacity of the venous system, the same observation is applicable. Though in a few rare and curious cases it is possible to recognize some circumstance which may operate in diminishing the area of the *sinuses* and the venous tubes of the brain, in a much larger proportion the difficulty of this is daily felt by the anatomist.

Under the fourth head, that of unusual inanition of the vascular system in general, and of that of the brain in particular, Dr Abercrombie advances several ingenious conjectures, and illustrates them by reference to cases. Whatever be the precise and intimate state of the vessels in cases of this description, we have had occasion more than once to witness the general accuracy of these observations remarkably verified.

Of these observations the result is, that in tracing the origin of hemorrhage, or rather we should say of *hemorrhagic injection* of the brain, much remains unexplained; and the attempt to give any thing like a rational or sufficient theory is beset with inconsistencies so innumerable as to perplex the more accurate inquirer, and difficulties so great as to seem almost insurmountable. This evil, indeed, is not peculiar to the pathology of the cerebral circulation. But while it shares this in common with the vascular system of every texture in the animal body, it possesses peculiarities, which, if we knew their exact influence, may tend remarkably to increase the difficulty of perceiving how the derangement is effected. While we agree with the ingenious author in admitting that the absolute quantity of blood within the cavity of the scull may not be increased, we must admit our incapacity to perceive that this is the question at issue; and it is further very questionable, whether that of the balance of circulation between the arterial and venous tubes is requisite in the investigation. In all the instances of deranged circulation in the textures within the cranium, as elsewhere, whether terminating in inflammation or its products, serous effusion, or bloody extravasation, the morbid state consists in an inordinate quantity of blood being in the capillaries of the organ or texture. These capillaries are, anatomically speaking, neither arterial nor venous tubes; but an extensive and multiplied net-work of innumerable minute canals, connecting the two systems, and partaking the characters of neither. The question, therefore, comes to be, not what subverts the balance between the circulation of the arterial and the venous tubes, but what are the circumstances under which the capillaries of the part become inordinately filled and distended with blood? What are the agents, in short, by which these capillaries are thus injected? Whoever answers this question resolves the problem. Whoever does not, must be held, we imagine, to fail, as all others have failed, in explaining the mode in which derangements of the capillary circulation of the substance of organs take place. Our inability we willingly admit.

It may be remarked, nevertheless, before we quit the subject entirely, that two points in the investigation may seem to merit some notice. One is, whether the blood moves more slowly than in the state of health, or, as some have imagined, entirely stagnates in some of the vessels? The other is, whether the large vessels and trunks contain their natural proportion of blood, or less than that; whether, in short, they are robbed to supply the distension of the capillaries? The first of these questions is no doubt connected with the circumstance of distension, and perhaps may prove to be either a cause or an effect of it. The second, however, is not necessarily connected with it, and may be safely thrown aside altogether, so far as we at present understand the inquiry. These questions this is not the place to prosecute further; and we proceed to the third general division, that of organic diseases.

Of these Dr Abercrombie does not attempt any arrangement. They may be conveniently distinguished into organic changes, incident 1*st*, to the membranes, and 2*d*, to the brain.

The membranes may be simply thickened, and somewhat indurated. The occurrence of this, from deposition betwixt the laminæ of the *dura mater*, is the only example mentioned by the present author.

A whitish semipellucid substance, presenting the character of albumen, may occasionally be seen in undefined masses beneath the arachnoid. A substance not dissimilar, generally attached to the *dura mater*, or some of its productions, may perhaps be referred to the arachnoid covering which extends over the free surface of that membrane.

To the same head belongs the fungous tumour of the *dura* *mater*, the history of which has been investigated at great length, and with much accuracy, by M. Louis,* and other authors. Of this examples are common in collections. Patches of ossification are very frequent in the *dura mater*, and, when found in the brain, are generally connected with the membranes or its productions.

Of tubercles in the *dura mater*, a good example is given by Dr Hooper in his sixth and seventh engravings.

Tubercles are found in the *pia mater*, and especially its cerebral prolongations.

That the cerebral substance is liable to induration as a consequence of chronic inflammation has been already noticed. Next to the cases of Morgagni, Meckel, and a few by Haslam, the best examples of this change are given in the *Annuaire Medico-Chirurgicale*, by M. Hebreart, a late eminent physician of the Bicêtre.† From these cases it appears that the portion of brain becomes firm, lardaceous, and of a yellowish tint, and loses entirely the usual appearance and peculiar atomic structure of cerebral substance.

Tubercular disease occurring in the brain may often be traced in its origin to the membranes. Thus, in the 79th case, p. 173 of the present author, a large tubercular mass, imbedded in the substance of the right hemisphere, was found attached to the falx, in which it has doubtless originated. In the twelfth engraving of Dr Hooper, in like manner, is given an example of white tubercle projecting into the fourth ventricle, which appears to be attached to the choroid plexus; and in the case described by Dr Robert Evans in the present number, the tubercular disorganization of the right lobe appears to have commenced in the *pia mater*, while that of the left belongs more distinctly to the cerebral substance, without and above the ventricle. In the first four of the present author's cases, the tubercular disease was confined to the substance of the brain or cerebellum, without involving the membranes. From these cases, it appears in general to consist of a mass or masses of round or spheroidal bodies of various sizes, aggregated together, firm in general, like cheese, and of a white or yellow-white colour. The smallest, varying from a pin head to a pea, are generally connected with the membranes. When larger, that is from the size of a pea to that of a walnut, they are usually imbedded in the substance of the brain or cerebellum. The reader finds their principal characters, anatomical and pathological, well illustrated in the Appendix to Part First.

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* Mémoires sur les Tumeurs Fongueuses de la Dure-Mere, par M. Louis. Caez Mémoires de l'Academie Royale de Chirurgie, T. xiii. p. 1. Paris, 1774.

† Observations sur quelques maladies du cervelet, du cerveau, et de leur membranes, recueillies à l'Hospice de Bicêtre, par M. Hebreart, Medecin Ordinaire des Alienés, &c. Annuaire, p. 579.

A tumour of a reddish or flesh-colour, and resembling the substance of the kidney, is represented by Dr Abercrombie as found in the brain. It is probable that he alludes to the blood-tumour, (*hæmatoma*,)* as this is nowhere mentioned in the present classification. The steatomatous and adipose tumours are simply mentioned.

Bony matter, when found in the brain, is almost invariably the effect of osseous deposition taking place in the membranes. Of this manifold instances are given by Bonetus, Morgagni, Lieutaud, Sandifort, and others. The only authentic instance which seems to be one of bony matter found in the substance of the brain unconnected with the membranes is that delineated by Dr Hooper in the seventh figure of his twelfth engraving. It is unfortunate that the history of this is not given at length; as in this manner only its connection with the membranes can be determined or disproved.

Under the general name of hydatids, Dr Abercrombie notices the hydatiform state of the choroid plexus, but properly remarks, that they are merely the cellular texture of the membrane elevated into vesicles by watery effusion. The real hydato-cyst, or globular tumour containing watery fluid, he also notices, and illustrates its nature by reference to the case of Mr Headington, formerly published in this Journal, and now in the appendix. For a good representation of these we refer the reader to the thirteenth and fourteenth engravings of Dr Hooper.

In the fourth part the diseases of the spinal chord and its membranes are considered in the following order. 1. Acute inflammation of the membranes; *(meningitis spinalis.)* 2. Inflammation of the chord itself, (*myelitis*,) terminating in pulpy destruction or suppuration. 3. Serous effusion in the spinal canal. 4. Extravasation of blood in the spinal canal or spinal apoplexy. 5. Fungoid disease and thickening of the membranes. 6. Induration of the chord. 7. Compression of the chord by new formations within the canal, as tubercles, albuminous deposition, hydatids, and ossification of the membranes. 8. Destruction of a part of the chord. 9. Concussion; and 10. Certain affections of the bones of the spine.

Though each of these forms of disease is well illustrated by apposite examples, it is unnecessary here to enter into minute examination of their character and history. Very analogous, if not entirely similar to the morbid processes already mentioned as taking place in the brain and its membranes, any lengthened examination would only lead to tedious if not profitless repetition. On one or two subjects only a few remarks may be offered.

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* Vide Howship's Observations in Morbid Anatomy, Case 27. Hooper, Plate X. Also the Morbid Anatomy of the Human Brain, by Alexander Monro, M. D.

The cases of pulpy destruction, (*ramollissement*,) of the chord, given by the present author, agree very closely in all essential characters with those published by Pinel Junior, the first accurate writer on this disease, and those of Olivier and Velpeau, the most recent next to Dr Abercrombie. The facts connected with the origin and progress of the disease show clearly that it is an effect of inflammation. Yet this inflammation does not appear to be necessarily fatal, at least when seated below the cervical portion of the chord; and when death ensues, it seems to do so indirectly and from another cause, or after it has destroyed a considerable extent of the chord.

The subject of hemorrhage into the spinal chord, or upon its surface, constituting spinal apoplexy, appears to have escaped notice, till the publication of the two interesting cases by M. Chevalier in the third volume of the Transactions of the Medico-Chirurgical Society. In the first of these which, so far as can be learnt from the report, was spontaneous, or without external violence, the hemorrhage was meningeal, since the blood was found between the membranes and the chord. In the second, which appears to have succeeded injury, the hemorrhage was both from the membranes and the substance of the chord, since the former was found inflamed, and the latter dissolved in blood. To these cases Dr Abercrombie has added one from Morgagni, one from Du Hamel, one from Gaultier de Claubry, one from Olivier, and one from personal experience. The case from Howship, which was occasioned by injury, has been since more fully published in the work of Sir Astley Cooper, on dislocations and fractures, as an example of the effect of injury in producing inflammation and hemorrhage within the spinal canal. It was formerly noticed in our account of that work.* The case of Du Hamel is the one which Sauvages, in an unhappy hour, ranked under the head of *asphyxia spinalis*, and thereby allowed the influence of an ill-chosen name to mislead the physician on the true nature of the disease.†

To collect these scattered examples of this variety of hemorrhage is a service of the utmost importance, for two obvious reasons. In the first place, the features of these cases, by their mutual resemblance, throw a clear and steady light on the pathological characters of the disorder; and by placing them in the same rank with hemorrhagic maladies in general, and those of the brain in particular, enable the physician to understand much more distinctly the pathological relations of an affection which is obscure, indeed, when viewed in an isolated position. Secondly, this verification of the character of spinal hemorrhage leads directly to the appropriate treatment. To M. Chevalier, indeed, belongs the merit of showing, that the disorder, to be treated with the hope of success, should be treated promptly and decidedly as a hemorrhagic disease.

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* Medical and Surgical Journal, Vol. xxi. p. 396.

† Nosologia Methodica, Classes vi. Ordo iv. gen. xxiv. 15, p. 820.

The fifth section, on fungoid disease of the chord and membranes, the sixth on induration, and the seventh on new formations compressing the chord, correspond entirely to the head of organic changes, and might with propriety have been arranged together. On the subject of concussion and injuries in general some interesting information is given; and in the section on *caries* and other unsound states of the bones, the reader will peruse with satisfaction various instructive examples of affection of the odontoid process of the second vertebra.

With all the elaborate research and generalization of the present author, nevertheless, it is highly perplexing to learn, that in several cases, in which all the usual symptoms of disorder of the chord or its membranes had appeared and continued to the last, neither in the brain nor in the chord could be found any change adequate to account for the symptoms. These anomalies and contradictions Dr Abercrombie does not attempt to explain; and it is superfluous for us to say, that where he does not attempt we cannot hope to succeed.

The appendix to this part contains a short general sketch of morbid states incident to the nerves. This, of course, is not offered for serious criticism; and it must be received by the reader rather as a guide to indicate the leading points of inquiry, than as a comprehensive source of information.

One of the most interesting parts of this inquiry is the knowledge of the history and effects of those minute tumours which are not unfrequently found in nervous trunks, the *subcutaneous tubercle* of Mr Wood, and the *neuroma* of Chaussier. The excruciating pain which these growths produce is perhaps the least of their evils. Fully a hundred years ago (1720) Dr Thomas Short of Sheffield showed that a minute tumour of this description, near the lower end of the gastrocnemii was adequate to cause epilepsy; * and though this was not observed in the cases of Cheselden, Camper, Bisset, Pearson, Home, and Mr Wood, the accuracy of the observation was recently verified by Mojon and Covercelli,* both of whom found these tumours connected with epileptic fits. It is not improbable, that, as in these cases the peculiar sensation called the *epileptic aura* was felt, the presence of these morbid growths was the material cause of the sensation. Though this is submitted as mere conjecture, we think it is not unseasonable to direct the attention of the practitioner to the fact, as it may enable him to cure a certain class of epileptic cases with more certainty than he could otherwise do.

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* Medical Essays and Observations, Vol. vi.

These remarks, however, it is time to close. Though they have extended to a much greater length than was at first contemplated, we are perfectly aware that they are neither sufficiently lengthened nor minute to do the work of Dr Abercombie proper justice. Of the arrangement we have already expressed our opinion; and this we do with less scruple, first, because in a work, the materials of which are good, defective arrangement appears much more conspicuously than in one of moderate merits; and, secondly, because we feel convinced, as the reasons must be admitted to be satisfactory, that the learned author has too much candour to be offended at our sincerity.

With this single exception, it gives us pleasure to say, that the merits of the work are so substantial as to render the ordinary terms of commendation quite superfluous. The diligence with which the author has laboured to illustrate the obscurities, and reconcile the contradictions with which the subject of diseases of the brain has been, and continues to be, surrounded, entitles him to the gratitude of every physician who wishes to understand one of the most important subjects of his profession. The examples of morbid action, and of its effects which he has recorded in the present volume, both from personal experience and the observation of his predecessors, possess an intrinsic value superior to the casualties of time, place, and opportunity. The importance of these the student of pathology will find little difficulty in recognizing; and he will speedily discover, that no individual work in the English language contains a selection of information so useful and judicious upon a class of disorders, upon which the most experienced physicians have daily occasion to deplore the want of precise and accurate knowledge.

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* Memorie della Societate di Genova, und Siebold Chiron, Band i. St. 3, where the Memoirs of Mojon and Covercelli are translated.

ART. II.—*Ueber die Punction des Chronischen inneren Wasserkopfs, &c.*

On the Operation of Puncture in Chronic Hydrocephalus. By DR FREDERICK WILLIAM OPPENHEIM, Physician and Surgeon, Hamburg. (*Rust's Magazin für die gesammte Heilkunde*, B. xxiv. 1827.)

IN publishing an additional instance of an attempt to cure chronic hydrocephalus by the operation of puncture, Dr Oppenheim has laboriously collected and analyzed all the cases of the kind previously made public, and by collating them, and comparing their results with the opinions of the best modern authors, has endeavoured to discover how far this mode of treatment deserves to be countenanced. He has in consequence arrived at the conclusion, that the particulars of the cases are by no means discouraging; that the objections stated by the writers who have discountenanced the operation are none of them very weighty; and that, if resorted to at an earlier stage of the disease, and under less desperate circumstances than in the instances in which it has hitherto been tried, there is reason to expect that it will not so often disappoint the hopes of the physician. The importance of Dr Oppenheim's opinion, and the want in our own language of an analysis such as he has drawn up, will render the following short review of his paper not unacceptable to the English reader.

We shall commence with an account of the author's own case, and an abstract of the analysis he has given of the rest, fifteen in number, which he has collected from various quarters.

The subject of Dr Oppenheim's case was a female infant 27 weeks old, who enjoyed good health for the first five months, but then began to exhibit symptoms of hydrocephalus, with enlargement of the head. At the date of the operation the head measured 18 inches at its greatest circumference, 11¼ from the root of the nose to the occipital protuberance, 7¾ from one parietal protuberance to the other, 13¼ from the *foramen magnum* to the *alæ nasi*. The sutures were separated considerably, with the exception of the frontal; the eyes were so much turned downwards that the pupils could not be seen. Before the puncture was made, the head was surrounded horizontally by a circular bandage, having attached to it at right angles four others, of which two were carried round the arm-pits forward and tied on the back, while the others were carried round the arm-pits behind and tied over the breast. The purpose of this arrangement was to afford points of resistance to eight linen straps which were attached to the upper edge of the circular bandage, and which, going through eight holes in a round piece of strong linen laid on the vertex, could be tightened at will by being pulled downwards and fastened on the circular bandage. This very convenient and effectual apparatus for maintaining a steady and general pressure being properly secured, the first puncture was made on the left side of the anterior fontanelle with a trocar and canula half a line in diameter; it was made to penetrate an inch obliquely; and an ounce and a half of a clear serum were withdrawn. The canula was then closed with wax, and secured in its place by plaster, and the bandages were properly tightened. The infant did not seem to suffer any pain, slept well in the course of the afternoon, and sucked vigorously. On the *second* day it was found that a good deal of fluid had trickled out, in consequence of the mother moving the child too much. Nevertheless two ounces more were withdrawn, and the bandages tightened. On the *third* day the child had several attacks of vomiting after taking powders of calomel and foxglove, which had been prescribed the evening before; but the head was diminished in size, the pupils were visible, and they acted readily. Serum had oozed from the wound. Three ounces were withdrawn by the canula, and the bandages again tightened. On the *fourth* day the canula was expelled during a fit of vomiting, the child was feverish, and the puncture had suppurated. The puncture was therefore closed with plaster, and cold fomentations were applied to the head. On the *fifth* day the fever having disappeared, a puncture was made on the right side of the fontanelle, fresh bandages were applied, and two ounces and a half of fluid were withdrawn. The cold fomentations were continued. On the *sixth* day the child was pale and motionless, hardly breathed, and refused the breast. The canula was therefore removed. On the *seventh* day it expired.

On dissection the greatest circumference was found to have been diminished two inches; the dura mater was very little thickened; the lateral ventricles greatly distended with fluid, the fourth ventricle natural; the brain soft and pulpy, its cortical not distinguishable from its medullary part; the cerebellum very soft and pulpy; the vessels at the base of the brain natural; the two punctures in the dura mater open, not inflamed, *and the trocar had not penetrated deeper* (?); above a pound of fluid was contained in the cavity of the skull.

Such is the substance of our author's narrative. The annals of surgery do not contain a history more unfavourable to the operation.

The cases which he has collected from other writers are fifteen in number. Two, however, are mere notices, and may be left out of the account. In four of the remaining thirteen the death of the patient was evidently more or less connected with the operation; for in three of them it took place within ten days after it, and in a fourth the child appeared to be exhausted by hemorrhage. In the other nine cases there was in general marked relief and abatement of the symptoms after each tapping, and the patients lived from 31 to 172 days after the first; in none was death distinctly connected with the operation; in several there was, in the opinion of the respective practitioners, a fair promise of ultimate success; and, according to Dr Oppenheim, one recovered; but this statement we shall presently find not to be correct.

The four unfavourable cases were the following. 1. Lecat operated on a child three months and a-half old, affected with hydrocephalus from the tenth week after birth. The sutures were considerably separated, and the eyes squinted. He took away five ounces thrice in three days, a canula being kept all the while in the opening; and the child died on the fifth day after the first puncture. The ventricles of the brain were much expanded, and the pineal gland nearly all destroyed.—(*Phil. Trans.* xlvii.)

2. Dr Whitmore of New-York, in similar circumstances, punctured a child six months old, in whom the disease began in the sixth week; and in eight days drew away, at fifteen different periods, from one opening, without a canula, 116 ounces. The child died two days after the last discharge of fluid; and the membranes were found inflamed.—(*American Med. Recorder, July* 1821.)

3. Dr Hood of Liverpool punctured a child nine months old, who had been affected since the third week after birth. Six ounces were withdrawn by a trocar, more oozed from time to time out of the wound, and death took place on the third day. The brain was softened, and the *tubercula quadrigemina* had passed into suppuration.—(*Edin. Med. Surg. Journ. October* 1821.)

4. Mr Brown of Preston operated on an infant five months old, and at first drew off nine ounces and a half. For 28 days afterwards the mother would not allow the puncture to be repeated. In the course of the eight succeeding days three new punctures were made, and from two of them fluid was withdrawn twice. On these five occasions 25 ounces were evacuated. Two of the punctures caused hemorrhage, one of them to a very great amount, and the child seemed to die of it namely, 36 days after the first operation.—(*Lond. Med. Phys. Journ.* li.)

The nine more favourable cases may be arranged according to the length of time they survived the first puncture.

5. Mr Gray operated on a child nine months old, whom at the age of one month he had cured of *spina bifida* by compression, but who was attacked with hydrocephalus not long afterwards. At three tappings, with an interval of ten days between them, he drew off 45 ounces. The child died 31 days after the first. The symptoms of pressure were mitigated considerably after each. (*Lond. Med. Phys. Journ.* liv)

6. Dr Frekelton of Liverpool operated on a child eight months old, who had the disease for three months. He employed successive punctures with a trocar, drawing away on the 1st day eight ounces, on the 2d seven, on the 7th two, on the 14th and 21st seven, on the 56th sixteen. Death took place on the 59th, and the brain was found expanded with 48 ounces of fluid.—(*Edin. Med. and Surg. Journal.*)

7. Mr Callaway operated on a child five weeks old, and drew away on the 1st day four ounces, on the 14th, 21st, 28th, and 35th, about as much ; no bad symptom followed any of the punctures, the head evidently decreased in volume, and the hearing and appearance of the countenance improved. Nevertheless the child died of gradual exhaustion and marasmus about the 70th day.—(*American Med. Recorder, July* 1821.)

8. Mr Remmett operated on a congenital case two months after birth. In the course of the first six days he punctured the head thrice, and drew away 36 ounces in all; four weeks afterwards he drew away 12 ounces, and four weeks after that 32 ounces. Till three weeks after the last puncture the child did well, but it then fell into general ill health, and died in ten days, consequently 93 days after the first operation. The punctures always healed two days after they were made.—(*Edin. Med. Comm.* vi.)

9. Mr Money operated also on a congenital case ten months after birth. On the 1st day he withdrew four ounces, on the 4th two drachms, on the 7th five ounces, on the 13th three, on the 21st three and a-half, on the 27th three, on the 46th four, on the 55th three and a-half, on the 66th four, and on the 74th two ounces. The child died on the 84th day, and the brain was found rather soft and dilated into a sac which held 35 ounces. Till the fifth tapping, the operation was followed each time by alarming faintness.—(*Lond. Med. Phys. Journ.* lii.)

10. Mr Sym of Kilmarnock operated on an infant eleven weeks old, that had been five weeks ill, and in whom the integuments were so tense that sloughing was dreaded. In the course of 90 days he punctured the head five times, drawing away seven ounces at an average each time Temporary relief was always

obtained, and after the last puncture the water ceased to accumulate. Fourteen days afterwards, or 104 days after the first puncture, the child died; and the membranes were found thickened, and two pounds and a-half of fluid within the head.—(*Edin. Med. Surg. Journ.* xxiv.)

11. Dr Glover of the United States operated in a case, apparently congenital, nine months after birth. In four months 156 ounces were withdrawn at eight tappings. Much temporary and considerable permanent amendment was procured; nay, after the last tapping the patient had strength to pass prosperously through an attack of cholera; but a continued fever afterwards made its appearance, and death took place on the 120th day. The dura mater, though thick, was not inflamed.—(*New-York Med. Repos.* iv.)

12. Mr Lizars operated on an infant seventeen weeks old, affected with the disease since its sixth week. In the course of 90 days he punctured the head sixteen times, at intervals of three, four, or seven days, and drew away each time from three to seven ounces; once only he withdrew ten ounces. After the last puncture on the 90th day, the convulsions with which the patient had been occasionally attacked ceased to return, and altogether matters promised well. About this time the irritation of teething seemed to be the cause of the prospect changing. On the 171st day, fluctuation having returned, three ounces and a half were withdrawn with temporary relief; but the convulsions returned, and death ensued next day. The brain formed a sac containing three pounds of fluid.—(*Edin. Med. Surg. Journ. April* 1821.)

13. Dr Vose of Liverpool operated on a case, apparently congenital, seven weeks after birth. At the first tapping he withdrew three ounces and a-half, on the 16th day five ounces, on the 38th eight, on the 47th twelve. After that the head did not increase, the sutures ossified, and a watery diarrhœa of four days' continuance appeared to carry off the remains of the disease. Three months later, when the author's account was drawn up, the patient continued well; and accordingly this case has been set down by Dr Oppenheim as an instance of recovery. We are informed, however, by Dr Monro (Morbid Anatomy of the Brain, p. 146,) that symptoms of pressure manifested themselves after the sutures were ossified, and the child ultimately died,—under what precise circumstances we have not been able to learn.—(*Lond. Med. Chir. Trans.* ix.)

We have now to consider the commentary and conclusions which Dr Oppenheim has formed on reviewing these cases. We shall not follow him through his enumeration of the opinions of authors on the subject. It matters little what were the sentiments of any writer, except in recent times; because till lately they were founded solely on the results of cases in which the whole fluid which would issue was withdrawn at once,—cases that appear to have uniformly proved fatal very soon after the operation. As to recent authors, by far the greater number either pointedly forbid it, or represent it as holding out the faintest possible prospect of success. Such, however, is not the opinion of Dr Oppenheim.

The objections which have been urged against the practice, and which he has taken under consideration, are the following. 1. That the mechanical injury done to the brain and its membranes is apt to induce inflammation; 2. That the entrance of air into the inside of the skull may have the same effect; 3. That much immediate danger may flow from the collapse of the brain; 4. That the operation has never been successful, or at least that the cases of that description are not more numerous than those of spontaneous cure. The last proposition is not very distinctly laid down by him, but he discusses it in the course of his reply to the objections.

In this reply he insists, 1. That the brain and its membranes have been repeatedly injured in a state of health without any material bad consequence; and in particular that none of the cases of puncture for hydrocephalus proved fatal by inflammation; 2. That with ordinary precaution the air may easily be prevented from entering by the wound, and that, although it does enter, it is not so injurious as has been alleged; for in Mr Lizars's case, in which it did effect an entrance, no material ill effect ensued; 3. That the danger arising from sudden collapse of the brain, certainly a real one, may without difficulty be obviated by withdrawing the fluid gradually, and making corresponding pressure by a proper bandage; and, 4. That spontaneous cures are so rare that his countryman Zang, who opposes the operation on the ground that such cures are as common as cures by puncture, could not say he had ever seen any; and Dr Baillie of London only knew of one case, and that an equivocal one.

He then goes on to observe, that, according to the results of the cases he has collected, the operation does not in the least disturb the vital functions, and in particular does not injure the brain or its membranes; that the brain, even in its internal parts, is tolerably insensible; that the size and form of the brain do not contribute to its functions; that at least one case, that of Vose, is on record, in which a cure was effected; and that the prosperous result in that instance, and the obvious amelioration produced in many others, although in all the disease had arrived at an advanced and desperate stage, entitles the practitioner to expect a more fortunate result from the operation at an earlier period. In fine, he maintains that, the disease being otherwise inevitably fatal, it is the duty of the physician not to exhaust the patient's strength in unavailing trials with internal remedies, but to resort to puncture of the head *as soon as he has satisfied himself that effusion has taken place.*

It does not appear to us that the author's statement of objections is altogether fair, or his arguments against them sound, or the facts on which he rests in forming his conclusions correct. We shall close this analysis therefore by laying down what we conceive to be the leading obstacles to the adoption of the proposed method of cure.

The objections drawn from the risk of air entering the cavity of the skull, or of the brain collapsing, or rather, we should say, of the brain being deprived of the pressure necessary to the right exercise of its functions, are certainly immaterial: They may both be avoided by simple precautions.

It is also certainly true, as Dr Oppenheim argues, that the brain and its membranes have, while in their natural state, sustained injury without any particular disturbance of the health. But he cannot be ignorant that the reverse is oftener the fact. Nor can his assertion, that puncture of a hydrocephalic head has never excited inflammation in the reported cases, be admitted as altogether accurate. Whitmore's case (No. 2) proved fatal two days after the operation, and the membranes were found inflamed; Hood's case (3) evidently ended in *ramollissement* and suppuration of the brain; it is probable that Lecat's case (1) was nearly of the same nature; and we cannot help thinking that Dr Oppenheim's own case likewise terminated in inflammation and softening of the cerebral and cerebellar tissue. Nevertheless, it is remarkable how frequently the brain and its membranes were pierced in the remaining cases without inflammation being caused; and it will presently be seen, that, in the four cases in which this result did ensue, the irritation to which the brain was subjected was unnecessarily great. On the whole, under proper management inflammation need not be much dreaded, much less than in many other surgical operations.

The fourth objection of the opponents of the operation, that it has never been successful, we maintain must stand uncontradicted. It has been already mentioned that the child, supposed by Dr Oppenheim to have been cured by Dr Vose, did really die at last of the disease; a short notice of a successful case extracted by him from Frorieps's *Notizen*, cannot receive from us more credit than is attached to it by our author, who thinks the circumstances too imperfectly related to justify him in adopting it; and as little can any sound inference be drawn from another short notice of a successful case by Rossi of Turin, which has been quoted by Dr Monro, but at the same time condemned by him as not sufficiently authenticated. (*Morbid Anatomy of the Brain*, p. 146.) There is then not a single well established instance of success on record. With regard to the amelioration produced in nine of the fourteen cases, it must undoubtedly be admitted, that the immediate effect of the operation was in every instance to relieve for the time the symptoms caused by the pressure of the fluid on the brain. But it may justly be doubted whether any permanent advantage was derived from it in more than five of them, (Nos. 5, 7, 11, 12, 13.) Nay, the reality of a permanent amendment even in them admits of doubt; for it is not satisfactorily proved with regard to any of them, by a relative measurement of the head, and of the progressive portions of fluid withdrawn, that there was a gradual decrease in the secretion of the fluid. After the punctures had been repeatedly performed, indeed, there happened in four of them (7, 10, 12, 13,) a sudden temporary arrestment of its augmentation; but this is no more than occurs in the ordinary course of hydrocephalus, and is not what would be expected to happen under the influence of a remedy, which is not merely palliative and subsidiary, but, as our author supposes, the principal and efficient means of cure. Did the operation strike at the root of the disease, as Dr Oppenheim seems to imagine it will, if resorted to at an early period, it ought to be seen acting gradually on what is, if not the cause of the fatal termination, at least the sure sign of its approach,—the increasing effusion. It ought to produce a gradual reduction of that increase,—an effect, which has not yet been clearly proved to occur in any of the published cases.

This observation leads to the remark, that Dr Oppenheim has omitted the most material objection to puncture as a remedy for hydrocephalus,—namely, that it is a mere palliative, that at best it puts off the fatal termination of the disease, by relieving from time to time urgent pressure on the brain, and that it does not in the least affect the derangement of function or structure, whatever that may be which gives rise to the effusion and other symptoms. This is apparent in all the cases to which it has been hitherto applied; nor can one easily see how it could be otherwise, although the operation were performed at the early period which our author insists upon. The operation therefore must be subordinate to the action of internal remedies, or the efforts of nature, in removing the tendency to increased exhalation. It is unnecessary to call to remembrance the little trust which can be put in art or nature for accomplishing this end, when unaided by the operation of puncture; it has been shown in the foregoing pages that the concurrence of the operation gives but little reason for increased confidence in the advanced stage of the disease; and it remains to be seen whether the physician will find himself better circumstanced by resorting to it at the beginning of the disease.

Although we entertain great doubts whether Dr Oppenheim is borne out in his sanguine hopes, we would by no means insist on the operation being abandoned. It may be well to conclude, therefore, with a few words of caution, as to the method of performing it, derived from the particulars of the cases which have been collected by the author.

The best method of performing the operation seems to be by means of a small trocar and canula, but the canula certainly ought not to be left in the wound, as was practised by Dr Oppenheim; there is evidently much less risk in repeated punctures; and it appears that all the cases, in which the canula was left in the wound, proved rapidly fatal. The punctures should not be frequent; when made once in four or seven days, they have always healed easily, without causing any irritation: On the contrary, when the fluid has been withdrawn frequently, either with or without fresh punctures, death has speedily ensued; when the secretion therefore is so rapid as to require very frequent punctures, the operation may be abandoned. The quantity of fluid withdrawn each time should not be great, not more, if possible, than from four to seven ounces. A single large tapping, as practised before Lecat's time, in general proves rapidly fatal, probably by the irretrievable removal of that degree of pressure which Magendie has proved to be necessary for the exercise of the cerebral functions. Finally, great care must be taken to keep up, by means of such a bandage as that of Dr Oppenheim, a steady gentle pressure of the skull, according as it diminishes in volume by the diminution of the fluid.

Art. III.—*The Anatomy of the Pelvis of the Male, illustrated by Nine Engravings of the size of Nature.* By Alexander Monro, M. D., F. R. S. E., President of the Royal College of Physicians, and Professor of Medicine, Anatomy, and Surgery in the University of Edinburgh. Folio. Second Edition, with additional Letter-press and Plates. Edinburgh, 1827.

THE importance of a correct knowledge of the situation, direction, and the mutual relations of the several parts contained within the bones of the pelvis, must be obvious to every one engaged in the practice of the healing art. It would be difficult to mention any region of the human body in which the physician, the surgeon, and the accoucheur, have so frequent occasion for the exercise of sound anatomical knowledge. Not only in the daily and ordinary practice of these arts do the physician and surgeon feel the value of this sort of information, but much more particularly on extraordinary occasions, in which, in consequence either of original malformations, of changes in the progress of disease, or other causes, the necessity of operation, manual or instrumental, becomes urgent. The necessity of this knowledge to the surgeon in the various operations of hernia, of cystotomy, of puncturing the bladder, and of managing diseases of the testicle and prostate gland, and to the accoucheur not only in delivering by instruments, but in performing various operations on the uterus and the adjoining parts, is too well known to require repetition.

The bad effects of the want of this species of information are distinctly evinced in the history of the lateral operation of lithotomy, and in that of the pathology of hernia. Most of the controversies about the manner in which Frere Jacques, and afterwards Rau, caused the knife to enter the bladder, originated in misunderstanding the relative situation of the parts at the neck of the bladder, and the manner in which an incision from the perinæum *must* pass, by the urethra, to this organ. Nor was the misconception confined to the mere surgeons of the day. Even such distinguished anatomists as Albinus, Foubert, Morand, and our countryman, Cheselden, entered into the controversy in a manner that seems unaccountable to those who understand the relative situation of the membranous portion of the urethra, the prostate gland, and the neck of the bladder.

Of its influence in retarding the progress of the correct pathology of hernia it is unnecessary to speak. They must be unknown to none who know any thing of the doctrines and practice of the surgeons of the beginning of the eighteenth century, and compare them with the information which the researches of Scarpa and Sir Astley Cooper have furnished.

It was chiefly under the influence of the conviction of the necessity of accurate anatomical knowledge of these parts, that Camper executed his magnificent engravings of the viscera of the pelvis, and the parts which are liable to become the object of surgical operation in this region of the body.* It was chiefly with the view of removing erroneous notions of this description that he collected the examples of intestinal protrusion, afterwards published by Soemmering. † It was chiefly with the same intention that Smellie, much about the same time, published his tables of the gravid womb.*

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* Petri Camper, Demonstrationum Anatomico Pathologicarum, Liber duo. Folio. Amstelaedam. 1760.

† Petri Camper, Icones Herniarum Cura Soemmering. Folio. Frankfort, 1801.

Since the time of Camper this department of anatomical science has been cultivated by different anatomists with different views and with various success. By his engravings of the gravid uterus, William Hunter superseded almost all previous and subsequent descriptions of the peculiarities of the female pelvis. By Edward Sandifort, Hey, Scarpa, Sir Astley Cooper, the present Monro, Allan Burns, Langenbeck, Breschet, and Mr Liston, the anatomical relations of the parts concerned in hernia have been investigated with various degrees of accuracy and minuteness. The subject of lithotomy had been so clearly explained by Camper that it was next to impossible to improve upon his description. Notwithstanding this, however, Mr John Bell first gave some good descriptions, but rather confused engravings; and Mr Charles Bell has at different times published very neat and showy illustrations of the parts concerned in the operation, and of the accidents and diseases incident to them. More recently still, the surgical world has seen the treatise of Mr Aston Key, which, though not equal to its pretensions, is still a work of value to the surgeon.

Among those who have distinguished themselves by their efforts to illustrate this important and useful department of surgical anatomy, the author of the present work is entitled to a very high rank. It is manifest, indeed, that he does not wish his publication to come exactly in competition with that of Camper, or the still more expensive one of Scarpa. But while, with the view of placing useful information within the reach of the surgical student, at a moderate rate, he has published his work in a smaller and less imposing form than these authors, his engravings, both as specimens of art and likewise as accurate delineation of parts, make a much nearer approach to those of the two anatomists now mentioned than any to which the surgical reader has access. It must not be understood, however, that the engravings are small. In each the parts are represented as large as life, though they are so arranged as to come within the compass of the folio. In noticing them more in detail, we shall make a few remarks on the merits and uses of each.

In the present work the most important circumstances of the anatomy of the male pelvis are placed before the student in nine engravings; and the principal objects of each, the mutual relations of the parts, and their application to surgery, are explained in accompanying description.

In a short but well-written introduction the author explains the utility of good engravings as an accessory guide to the student in the acquisition of correct anatomical information, as records of facts already ascertained, and as the means of reviving in the memory the fading traces of previous knowledge.

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* Anatomical Tables. By Dr W. Smellie, &c. Fol. London, 1759.

After some general remarks on the figure of the pelvis, and on its mechanical and physiological uses, and some short directions for the dissection of the muscles of the perinæum, the student is presented with a view of the pelvis of the adult female, with its ligaments, fixed and articular. The parts are represented in their recent state, that is, with the muscles and cellular substance only removed, so as to convey to the student a correct idea of the mechanical figure and capabilities of the lower aperture of the pelvis. As this aperture is bounded before by the *symphysis pubis*, behind by the sacrum, and on each side by the ischial *rami* and tuberosities, the attention is here directed to the relation between the aperture and the head of the fœtus, and to that between the interischial space and the diameter of a stone to be extracted by the lateral operation from the bladder.

In the second engraving is presented a very fine and distinct view of the muscles of the perinæum, and their relative connections with the bones, and the penis or urethra. In speaking of the *erectores* Dr Monro properly remarks, that these muscles tend to draw the penis downwards, and towards the ischial tuberosities. The truth is, these muscles are most improperly named; for instead of tending to the action from which they have been denominated, they have an effect exactly the reverse. Though this was originally noticed by Haller, and perhaps by others, their name and supposed uses have been continued in anatomical demonstration, to the confusion of every one who thinks on what he dissects. They are important in being the seat of the painful spasmodic action denominated *chordée* The *transversus perinæi alter*, in the subject from which the present engraving was taken, is reported to have been wanting. The fibres we have more than once found so indistinct as to make us think it had been destroyed by incautious dissection. The description is concluded by some good remarks on the direction of the incisions in the lateral operation.

The third engraving presents a most useful and instructive lateral view of the perinæum and the parts contained in the pelvis. The principal objects deserving attention are the situation of the muscles of Mr Wilson, that of the bulb of the urethra, the parts of the *levator ani*, one of the lobes of the prostate gland, one of the seminal vesicles, one of the *vasa deferentia*, the mutual relations of the several parts of the ilio-vesical fascia, the rectum, and the urinary bladder. Of the latter the muscular coat is represented with great neatness and distinctness; and from personal dissection of the same parts we can most truly say that they are delineated with great accuracy. In short, this engraving, which is perhaps the best in the collection, is altogether a very beautiful specimen of the application of the arts to anatomy. As the only one in which this lateral view of the pelvic organs is given, it is further entitled to the particular attention of the surgeon.

The fourth plate presents three separate figures, which, though done in a different style, are nevertheless excellent delineations of the objects. We must first, however, notice an error in the references; the figure marked 1 being really the one described in the letter-press as the 3d; and, conversely, that which is marked 3 being in truth the first. Having adjusted this point, we have only to say that the second figure presents a view of a piece of anatomy highly important to the surgeon, the plexus of large veins lying between the bladder and rectum, and forming a multiplied net-work on the upper surface of the prostate gland. It is rather a judicious conjecture of the author that the bleeding which occasionally happens after lithotomy may take place from these veins more frequently than from the pudic artery. This must occur, however, chiefly when the prostate gland is improperly divided, and especially in old subjects, in whom these vessels are almost uniformly much dilated, and approaching to varicose. It was with the view of avoiding this source of hemorrhage as much as may be, that Mr Key advises not to slice off the upper part of the gland, as is sometimes done, especially by the gorget, but to cut it fairly through the lower part of the left lobe. The 3d figure, which is a copy from Camper, and which it has been already noticed is by mistake marked 1, gives a correct view of the situation and course of the blood-vessels and nerves of the pelvis. The various origin and situation of the obturator artery, in reference to the operation for strangulated hernia, is the subject of particular consideration.

The fifth plate, which is done in the same style of lithography, consists of four figures, illustrating several points relative to possible casualties in the operation of lithotomy. In the second figure is exhibited a peculiar deformity or malformation in the ascending branches and the tuberosities of the ischium on each side, in an adult male otherwise well made, in consequence of which it would have been impossible to perform the lateral operation, so as to extract a urinary concretion of moderate size. The *rami* of the ischium, instead of mutually receding as they descend, as in ordinary circumstances, appear to have been nearly parallel, so that at the lower part and the tuberosity the interspace was not larger than above. In the third figure are seen the pudic arteries and nerves; and the author takes occasion to point out the course of the vessels, in reference to the incisions. These figures, though somewhat stiff, are nevertheless distinct and instructive.

The sixth engraving, if not the best in the whole collection, is an excellent specimen of the art. Infinitely superior to any of those usually found in works either of anatomy or surgery, it reminds the admirer of art of the engravings of Anderloni in Scarpa's works much more closely than any thing which he has an opportunity of seeing in anatomical plates. It represents distortion of the pelvis from the same subject apparently as that which furnished the second figure in the preceding engraving, the state of parts in *prolapsus*, and several of the muscles, blood-vessels, and nerves of the perinæum.

The seventh, which is also very good, is copied from Scarpa, and exhibits very beautifully, as every thing of that eminent anatomist does, the parts concerned in the production of inguinal and crural hernia, and those concerned in the operations for inguinal and femoral aneurisms. Of the merits of this it is unnecessary to speak. It is sufficient to say that Dr Monro has succeeded in laying before the reader an exceedingly faithful and beautiful copy of the original. The description contains some valuable observations on the origin and distribution of the epigastric artery, and its situation relative to the hernial sac, and on the modes of securing the femoral artery.

The eighth engraving, which is original and very well executed, gives a direct or orthographic view of the perinæal arteries, already represented in profile in the fifth plate. The student will find the *accelerator*, *erector*, and *transversus* muscles, and the trunk and principal branches of the internal pudic artery, very distinctly and faithfully represented. So far as we are aware, no view of this part of surgical anatomy is so clearly given as in the present engraving.

In the ninth plate is given a deviation from the usual distribution of the perinæal arteries, with which it is well for the surgeon to be familiar. This consists in an artery arising like the internal pudic from the trunk of the internal or posterior iliac, (*hypogastric artery*), and passing along the side of the bladder and prostate gland to become the dorsal artery of the penis. The difference between this and the common origin is, that in the latter the dorsal artery is a branch of the internal pudic, whereas in the former it issues directly from the internal or posterior iliac. In such circumstances the internal pudic, as in the case before us, is smaller than usual,—a circumstance, the reason of which is easily understood, when it is remembered that it has perhaps not one-half of the usual parts to supply with blood.

The occurrence of such a distribution of arteries as that now noticed unquestionably augments the danger of hemorrhage in the incisions made for cystotomy. In ordinary circumstances the vessels which *must* be divided are the perinæal artery, which is always cut along with the *transversus perinæi* muscle, and the transverse artery of the perinæum.* It occasionally happens, that, from carrying the knife too much towards the ascending branch of the ischium, the trunk of the internal pudic, near where it begins to quit the bone, is partially wounded; and this forms the worst source of hemorrhage in the ordinary distribution. In the extraordinary distribution given by Dr Monro in the present engraving, it is manifest that the vessel is very likely to be divided, if not entirely, at least obliquely. The entire division, it is probable, would be a source of hemorrhage less troublesome than the incomplete one; for if the vessel even be divided across, the hemorrhage would likely cease upon its contraction. In no case, however, ought this consideration to weigh on the mind of the surgeon as an objection to the lateral operation. Notwithstanding all that has been said about the danger of hemorrhage in this situation, it is impossible, anatomically speaking, to conceive any hemorrhage which the surgeon could not restrain. If the vessel cannot be tied, a piece of dry charpée or a portion of sponge carefully introduced into the wound, and applied to the bleeding orifice, can scarcely fail to favour coagulation, and form a barrier to the further effusion of blood. Instead of obtruding, however, our own observations, which this subject has led us to do, we must refer the reader to the work of Dr Monro, where he will find matter much more deserving perusal.

To conclude, though we are not partial to anatomical engravings—though we cannot say conscientiously that we ever studied any engraving with the sole view of learning the most trivial anatomical fact—though we have always derived our knowledge in this department from the subject, and shall continue to do so—though, in short, we advise no one to attempt operation upon the knowledge obtained from engravings alone;—we regard the present work as a most valuable guide to the student of surgery. It will both facilitate the acquisition and the preservation of knowledge; and it may be strongly and justly recommended to the attention of the practical surgeon.

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* For properly understanding the situation and distribution of these two vessels, which are not always accurately distinguished by English authors, we refer to the following descriptions. S. H. Soemmering *de Fabrica Corporis Humani*, Tom. v. § 190, pp. 280, 281. Traité d'Anatomie Descriptive, par X. Bichat, Tome iv. pp. 307 and 308. A Paris, An. xi. (1803.) Traité Complet d'Anatomie, par M. Le Baron Boyer. Tome iii. 4th edit. A Paris, 1815. p. 138 and 139, and Traité d'Anatomie Déscriptive, &c. par J. H. Cloquet, D. M. &c. Seconde partie, pp. 941, 942. A Paris, 1816.

ART. IV.—*Observations on the Mortality and Physical Management of Children.* By JOHN ROBERTON, Member of the Royal College of Surgeons, Edinburgh; one of the Surgeons to the Manchester Lying-in Infirmary, &c. 1827. 12mo.

THE pursuit of medical statistics is in this country unhappily attended with many difficulties, and is too apt to lead to unsatisfactory or uncertain results. Our public mortality bills, which must form the basis of all general researches, and to which all special investigations must bear a reference, are, if not so inaccurate as generally represented, at all events miserably defective. For want of a few additional particulars, which it would have been easy to collect along with the rest, the attention of the statistical inquirer must be confined in a great measure to the general facts of the science, and it is exceedingly difficult, if not impossible, for him to pursue it into its remoter corners, and to make those more special researches, by which chiefly we are to discover where the public health is deficient, and how it may be improved.

It certainly reflects disgrace on Great Britain, and no little discredit on our Houses of Parliament, that matters are in this respect allowed to remain so long in the same state. Every succeeding year teaches us to lament that our ancestors were ignorant of the great objects to be attained by extending the mortality bills. Yet with our eyes open we neglect the lesson, and seem disposed to revenge the loss we have sustained from our predecessors, by transmitting it to our successors unrepaired. The faint attempt at improvement in the statute of 1812 will hardly relieve the Legislature from the change of indifference. We fear it deserves too truly the reproaches heaped upon it by Mr Roberton. " The deviser of that act, says he, deserves little praise for his exertions. His plan is meagre and defective in the extreme. If the mode of parochial registration which had been so long followed required revisal, it should have been entered upon with full intelligence as to the nature of the task, and a sincere desire as far as possible to remedy every defect; qualifications of which no trace appears in the act in question." It is to be hoped, that amidst the extensive inquiries which are now making into various departments of the internal policy of the kingdom, this most important topic will not remain much longer unnoticed.

Meanwhile, by the activity of private individuals, and the public spirit of a few municipalities, the defects in the general system have been supplied for some local districts; and comprehensive tables have been kept, the examination of which has led to the discovery of some striking facts. These facts, however, are often lessened in value for want of comparative statements, and still more frequently for want of an analysis of their causes;—an inquiry, into which it is impossible to enter without tables of still greater comprehensiveness than any now existing.

Among the discoveries which have thus been brought to light, there is none more pleasing to the physician and the philanthropist than the almost inconceivable amelioration which the progress of physic and political improvement together have effected in the mortality of children. And we apprehend there are few discoveries in medical statistics which, when deeply examined, will lead to so many useful practical hints for directing us to farther advancement. This subject has engaged the attention of several writers, and in recent times particularly of Dr Watt, Sir G. Blane, Mr Mylne, and Dr Casper. In Mr Roberton's work it is handled in a very clear and satisfactory manner. His object is to arrive at an accurate numerical statement of the general fact, to trace it as far as possible to its causes, to correct certain errors committed by others in the latter investigation, to show how much our efforts at future improvement should have reference to the physical education of children, and to lay down the rules by which their physical education should be conducted. It consists, therefore, of two parts; in the former, or statistical part, he treats of the mortality of children and its causes; in the latter, of their physical education as regards their health. In the former he shows that he possesses no small share of the talent of the statistical philosopher; in the latter his instructions are judicious, popular, and with a few trifling exceptions correct. We shall give a short analysis of his statistical inquiries. The practical part of his treatise is too concise to admit of abridgement.

After a few remarks on the nature of mortality bills, and the defects of those kept in Britain, he gives a number of local tables from various parts of the country, and proceeds to determine the relative mortality among children in different places. From this inquiry it results that the mortality is much greater in cities than in small towns, and still greater than in the country. The tables, however, by which he endeavours to establish the numerical relation between them, are not exactly adequate to the end in view. He makes it appear that from the tables of London, Liverpool, Glasgow, and Manchester, the deaths under ten years of age are, on an average of the whole cities, 51.39 per cent. of the total mortality;—that in small towns, such as Chester, Carlisle, Warrington, and Northampton, whose population varies from eight to twenty thousand, the average per centage is 46.97;—that in village parishes it is 45.9;—and that in agricultural parishes it is so low as 35.4. Two objections lie against these results. In the first place, the tables from which the data are taken were constructed at very different periods, some only a few years since, others so long ago as the middle of last century; between which periods it will presently be seen that a vast change has taken place in infantile mortality. And secondly, as the total mortality is much greater in some places than in others, it follows that the mode of counting by the ratio of the infantile to the total deaths, does not convey an accurate idea of the number of deaths during infancy, in proportion to the population. This objection has been foreseen by the author; and the following example he has given of a result corrected on account of it, is an excellent illustration of the magnitude of the errors which the ordinary way of computing relative mortalities may introduce. For thirty years after 1782, in every hundred deaths at Glasgow, 54.75 were under ten. For twenty years after 1746, in every hundred deaths in the parish of Ackworth, an agricultural parish in Yorkshire, 27.33 were under ten. Whence it would be natural at first sight to infer, that the mortality among children was twice as great in Glasgow as in the country districts of Yorkshire. But in reality this is not a correct account of the ratio; for the *general* mortality was much greater in Glasgow than in Yorkshire. The numerical facts we shall state in a manner somewhat different from our author. In Glasgow the deaths annually were one in every forty inhabitants, in Ackworth one in fifty-six. Hence in Glasgow, among every thousand inhabitants, there died annually 25, of which 54.75 per cent. or 13.7, were under ten; while in Ackworth, among every thousand inhabitants, there died annually 17.8, of whom 27.33 per cent. or 4.8 were under ten. *Consequently for every child that died in the latter place, in a given number of inhabitants, three died in Glasgow.* This is an appalling fact. Probably, indeed, the places chosen afford extreme results. But unhappily we are not in possession of data for comparative observations.

The next subject to which Mr Roberton turns his attention is the causes of the great comparative mortality of children in towns over that in the country.

The causes he enumerates are those very obvious ones,—the foul air of crowded houses and apartments,—the consequent facility with which infectious diseases propagate themselves—poverty, and particularly in manufacturing towns, sudden alternations of ease and want—increased number of illegitimate births, the tendency of which to augment the mortality among children was very well shown in our account of the work of Dr Casper. Another cause of which Mr Roberton treats more specifically is the custom of the lower classes in towns of giving the youngest children pure spirits, an abominable practice, which in Manchester, it seems, the reluctant babes are gradually taught by making them in the first instance suck the finger dipped in the liquor. Another and a very important cause in the case of Manchester is the total want of vacant spaces throughout the town for public walks. It is to this cause that the author is disposed to assign the greater mortality among children in Manchester than in other towns of equal size, in which the general mortality is the same.

The next section treats of the improvement in infantile mortality which has taken place within the last forty or fifty years.

For proving this fact, and determining the improvement numerically, he has had recourse to the bills of London, Glasgow, Liverpool, Manchester, Warrington, Chester, Carlisle, and various country parishes in Lancashire and the adjoining counties. Few of these bills yield satisfactory results, as most of them are more or less defective. The following example, however, we shall select as being complete in all its parts, and made on a pretty extensive scale of years and population. In Warrington, a manufacturing town in Lancashire, of about thirteen thousand inhabitants, the average annual mortality for nine years subsequent to 1772 was one in 26.48, and of the deaths 55.12 per cent. were under ten: The average annual mortality for eight years after 1817 was one in 37.4, and of the deaths 44.65 were under ten. Consequently in every thousand inhabitants there died annually, of children under ten, 20.8 in the first period, and only 11.94 in the second. We shall presently find, from analogous data, that the improvement in Glasgow between the six years after 1782, and the six previous to 1812, was not much less, namely, from 20.03 to 13.58 in every thousand inhabitants.*

In the next section Mr Roberton proceeds to rectify an oversight which has been committed by Dr Watt of Glasgow in his well-known observations on the Mortality of Children. The subject of this section is one of very great importance.

Dr Watt, as many of our readers must be aware, inferred from an examination of the Glasgow tables, that in that city the mortality among children had *not* improved between the two periods mentioned above; and that, as a manifest diminution had taken place by the abatement of small-pox, this improvement must have been compensated by a corresponding increase of deaths from other infantile diseases, more especially measles. In a late number of this Journal we have inserted a pointed contradiction of Watt's statements by Dr Casper of Berlin. Dr Casper found from the statistical tables of that metropolis, not only that the mortality among children was really and materially improved in the course of thirty years subsequent to 1790, but likewise that the decrease of deaths from small-pox did not account for the whole improvement; nay, that in proportion to the births there was an actual diminution in the number of deaths from measles.* But Mr Roberton has succeeded in contradicting the conclusions of Dr Watt from the data which he himself has furnished. Dr Watt's statement, as to the permanence of infantile mortality in Glasgow, was founded on the fact, that for six years subsequent to 1782 the deaths under ten years of age were 53.48 per cent. of the total deaths; while in the six years concluding with 1812 they were 55.43 per cent. Unfortunately, however, for the accuracy of his inference, Mr Roberton shows that Dr Watt did not take into account the great improvement which has taken place during the interval in the *total* mortality, and omitted to make the requisite correction, namely, by referring the infantile deaths to the population, instead of the general mortality. Let us now see what will be the result of that alteration. In the early period, the average annual mortality was one in 26.7, and of the deaths 53.48 per cent. were under ten: That is, among every thousand of the population there died annually 37.45, *of whom* 20.03 *were under ten.* In the latter period the annual mortality was one in 40.8, and of the deaths 55.43 per cent. were under ten: Hence in every thousand inhabitants 24.51 died annually, *of whom* 13.58 *were under ten.* So that in reality the deaths among children in a given number of inhabitants had decreased to two-thirds of what they were in the former period. We are not aware that this satisfactory correction has ever been made before.†

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* See Note †, next page.

We shall now, however, take a wider view of the section in which the foregoing criticism is contained, and which treats generally of the alleged reciprocity between the deaths from small-pox and those from measles, scarlatina, and other infantile diseases. The author has been led to bestow some pains on the subject, as in Manchester the statements of Dr Watt and others have had a very pernicious effect on the advancement of vaccination. His account of the state of vaccination in that city is indeed deplorable. From May 1815 to May 1823, the annual number vaccinated at the Infirmary and Lying-in Charity was 3161. From May 1824 to May 1826, although the paupers who repaired to these establishments for other purposes were much increased, the annual number of children vaccinated has fallen to 1309.

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* xxvi. 177. In our analysis of Dr Casper's work an error has been allowed to pass unnoticed, which we take this opportunity of correcting. In page 178 line 10 it is said, "These statements of Dr Casper, which so strongly *confirm* those formerly made by Dr Watt." *Contradict* should of course have been the word.

† The mode of computation which refers the deaths in childhood to the population is much more correct than that which refers them to the general mortality. Nevertheless it is not quite exact. For when the mortality among children is improving in a greater proportion than the general mortality, which has for some time been the case every where, it follows that in recent times there must be a greater proportion of infantile population in the same number of inhabitants than in more remote periods. Now it is properly to the infantile, not to the general population, that the infantile deaths should be referred. But it is by no means easy to ascertain, from present data, what the proportion of children in the population amounts to.

We are enabled to place Dr Watt's statement of the increase of measles in Glasgow in a clear and satisfactory point of view from a document, which we regret Mr Roberton does not seem to have had it in his power to consult. The increase observed by Dr Watt in thirty years subsequent to 1782 was a twelve-fold ratio ; and even after making the proper correction on account of the error mentioned above, and referring the deaths to the population, there will remain an increase in the ratio of one to seven. This great rise does not correspond with the increase elsewhere : In London, for example, a much wider field of observation, the increase between 1791 and 1820 has been only in the proportion of two to five. We were therefore led to conjecture that the great apparent increase at Glasgow must have been owing to the extraordinary prevalence of epidemics towards the close of the period included in Dr Watt's table. And accordingly this will appear pretty evident from the table itself; for while the annual average of deaths from measles for ten years before 1813 was 166, the numbers in three of the six years which form the last period of the table, were 267, 304, and 787. But farther, on referring to the valuable statistical account of Glasgow published in 1820 by Mr Cleland, it will be found, that the ratio of deaths from measles to the total deaths,—which ratio had risen, during the thirty years comprised in Dr Watt's table, from 0.8 per cent. to 9.09,—was for four years subsequent to 1812 (the last year in that table,) only 4.19 ; for the next three years 3.0 ; or for the whole seven years 3.69. On consulting the table below, which we have calculated from the data furnished by Mr Cleland from Dr Watt's investigations and his own inquiries, the reader will perceive, that while the yearly deaths from small-pox in Glasgow have decreased, during the forty years prior to 1820, from 63.2 in ten thousand inhabitants to 4.0, those from measles have increased from 3.0 to 7.9 only. * Dr Watt's estimate of the great increase of

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* The subjoined table comprehends five periods of six years each, and one period of seven years. The numbers in the per centage department differ from those in Mr Roberton's table. We have verified their accuracy, however.

From 1783 to	Deaths from			Of 100 deaths, were from		In 10,000 Inhabitants there died yearly of	
	All Diseases.	Small-Pox.	Measles.	Small-Pox.	Measles.	Small-Pox.	Measles.
1788,	11544	1955	93	16.90	0.80	63.2	3.0
1794,	14174	2135	130	15.06	0.92	——	——
1800,	12950	1869	210	14.43	1.62	33.8	3.8
1806,	13584	892	403	6.56	2.97	——	——
1812,	15801	522	1437	3.30	9.09	7.95	21 .
1819,	22060	236	814	1.07	3.69	4. 0	7.9

measles is therefore distinctly vitiated by the prevalence of epidemics towards the close of the period to which his inquiries were necessarily confined.

But although the decrease of small-pox is thus even in Glasgow very far from being compensated by the increase of measles, there is no doubt that the latter disease in proportion to the population, although not more common, as appears from Dr Casper's inquiries, at Berlin, is materially so since the diffusion of vaccination both in London and Glasgow, and probably in Britain at large. Returning now, however, to our author, he maintains, that the increase, such as it is, arises from nothing else, than, on the one hand, from a greater number of children, who used formerly to be carried off by small-pox, being now left to be affected by other infectious diseases; and, on the other hand, perhaps, from an increased fatality among the infected, which is owing, not, as some imagine, to the small-pox formerly cleansing and strengthening the constitution so as to enable it to resist future diseases, but simply to these diseases now finding a greater number of unsound constitutions to attack, which in former times were previously disposed of by the small-pox. And he denies altogether that any sound argument has ever been brought forward to prove, that the want of those mysterious changes, which are supposed to have been effected in the body by small-pox, does now either engender or leave undisturbed in any constitution a liability to be either more easily or more severely affected by measles and other disorders. On the contrary, " he is inclined to think," and we believe with great justice, " that the truth lies in an exactly opposite conclusion, and that one considerable cause of the decrease of mortality of late years in every part of the kingdom, is *the comparatively greater soundness of constitution enjoyed, since small-pox has given way*."

In support of this opinion he mentions that consumption, which during last century had increased in a distressing ratio, appears to have begun to decrease at a date, after the general introduction of vaccination, corresponding with what would *a priori* be expected, considering the period of life at which the disease in question begins to prevail. This effect, if it has really taken place, cannot be expected to be yet very decided, because a sufficient time has not elapsed to bring a great number of the vaccinated to the age at which consumption is *most* prevalent. But still the improvement, judging from the London tables, is manifest, and certainly began about the period mentioned. At the close of last century the deaths from this disease had gradually increased during the previous hundred years from 15.4 to 26.3 per cent. of the total mortality. Between 1799 and

1808 they still continued to increase, being 27.8 per cent. But after that, namely down to 1808, they had decreased to 23.8; and between 1818 and 1825 to 22.2. As during the last thirty years there has been a great improvement in the general mortality in London, it is obvious, that, if the proper correction were made on this account, and the deaths from consumption taken in relation to the number of inhabitants, the improvement would be still more manifest. It is not easy to ascribe the decrease to an improvement in the political condition of the country, as many would be disposed to do; else why did consumption increase so much during last century?

The author might have farther illustrated his opinion, that the absence of small-pox does not render the constitution more liable to other diseases, by the fact, that the mortality above the age of ten from all diseases is lessened. We have already seen that the abatement of the mortality among children under ten is not nearly accounted for by the diminution of deaths from small-pox. It may now be added, that the improvement in the mortality under ten does not account for the improvement in the general mortality. Thus, taking the town of Warrington as an example: In the eight years subsequent to 1772, the total mortality was 37.7, and the infantile 20.8 in a thousand; while in the eight years previous to 1825, the total mortality was 27, and the infantile 12 in a thousand. Hence, for every thousand inhabitants, there died in the first period 16.9, in the second 15 above the age of ten, *being a diminution of nearly nine per cent. of such deaths.*

Let us now turn for a moment to the relative mortality among children of different ages, and to the relative change which has taken place among those of different ages in the course of the modern improvements in medicine and domestic life.

It is so well known that a vast proportion of the deaths among children occur during the first years of life, that it is unnecessary in this sketch to quote any of the fresh proofs to that effect which have been presented by Mr Roberton. But it may be interesting to notice a few of the facts adduced by him to point out the very small mortality among children between five and fourteen. About the year 1780 the deaths between five and ten in Warrington, Chester, and Carlisle were, for every 10,000 inhabitants, only *fourteen and a third*, *thirteen*, and *twelve*, neglecting small fractions. In the Manchester Blue-coat Hospital, which contains eighty boys between six and fourteen years of age, there have died in twenty-six years only four, or one in 520 annually. In the Warrington Blue-coat School, among twenty-eight children, between eight and fourteen, none have died in twelve years. At the seminary for the children of the Society of Friends at Ackworth, containing 300 children between ten and fourteen, there have died in four years only three, being a mortality of one annually in 400.

To these statements it may be interesting to add some corresponding facts derived from the kindred institutions of this city. Heriot's Hospital is an institution for the maintenance and education of boys who are the children chiefly of the better class of operatives and lower class of shopkeepers. Certain chronic diseases disqualify for admission. The age of the children is from seven to fourteen. For the last seventeen years the average population has been 180, and the deaths have been thirteen, making an annual mortality of one in 235. Watson's Hospital is a similar institution for the children of deceased or decayed members of the Merchant Company, which consists of the superior class of shopkeepers and of merchants. From 1778 to 1811 the population was 55 ; from 1811 to 1820 it was 70 ; from 1820 to 1828 it has been 80. The deaths during these fifty years have been twenty-five ; consequently the annual average mortality is one in 123.4. The boys are from eight to fourteen years of age ; and, as in Heriot's Hospital, certain chronic diseases disqualify. The nature of the Orphan Hospital needs no explanation. It contains boys and girls between the age of eight and fourteen. We have been unable to procure the particulars from which the estimate of the mortality is taken ; but we have good authority for stating it during the eight years previous to 1823 so high as one in 37.5. The High School and Academy, the grammar schools of the city, are attended by boys from seven to fifteen years of age, who belong almost entirely to the middle ranks ; and live with their parents or friends throughout the city. The population of the High School for the last six years has been on an average 680 ; that of the Academy since it was opened in October 1824 has been on an average 500. At the High School in six years *five* have died, being an annual mortality of one in 816 : at the Academy in three years and-a-half *two* have died, being a proportion of one in 875. *The annual mortality of both institutions embracing a population of* 1180, *is only one out of* 833. These interesting facts are all derived from the most direct and authentic sources. It is evident that they would lead to much curious inquiry into the causes of the extraordinary difference of the mortality at different institutions. *

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* The very small mortality thus indicated among boys of the middle ranks of society in this city cannot be imputed to delicate children being educated at home, and consequently not included in the enumeration in the text. In point of fact, parents here, far from showing a propensity to pamper delicate children in their nurseries,

It appears that the improvement in the total mortality and in infantile mortality, which has been produced in the course of the last thirty or forty years, has not affected much the mortality among children of the denomination we are now considering. In some places, indeed, one would be apt to suppose from the tables at first sight, that there was an increased mortality among them. Thus we find by calculation from the tables of Dr Watt for Glasgow, and of Mr Roberton for Warrington, that in the former place the deaths among children between five and ten for every 10,000 inhabitants had risen betwixt 1783, and 1812, from 12.8 to 13.7; and in Warrington, betwixt 1773 and 1825, from 14.3 to 19.3. This apparent augmentation, however, will be accounted for in the case of Warrington, and more than accounted for in the case of Glasgow, when it is considered that a ratio to the population is not altogether a fair way of calculating in the present instance. In fact, in late years, in consequence of the improved mortality among children under five, the proportion of children between five and ten to the total population has been very much increased. It is not easy to make the proper correction on this account; but it is plain that it will completely compensate for the apparent increased mortality in Warrington, and alter the apparent increase at Glasgow to a substantial diminution. Still it cannot be questioned that the improvement between the two periods of

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generally send them to school with the rest; and it is consistent with the experience of our teachers, that their delicacy frequently wears off very soon under grammar school regimen. No species of physical education appears better calculated for insuring health and hardiness. The boys have abundance of exercise of the most vigorous kind, and in all weathers; and at the same time, they are constantly under the watchful eye of those most interested in their welfare.

The mortality in Watson's Hospital calls for one or two remarks. It is about double that of Heriot's Hospital, although the age of the boys, and general system of management, are as nearly as possible the same. But in fact, even this ratio is not the correct one. During the first ten years of the half century, namely the ten years after 1777, the mortality was one in 137.5; *and in the last ten years it has been one in* 71. May not this unfavourable ratio arise from the straightened circumstances of the parents previous to the admission of their children; and more particularly their sudden transition from ease and affluence to comparative poverty,—a transition which will of course be much greater, and much more severely felt in their case, than by those of the class from which the population of Heriot's Hospital is supplied? Will not the increased degree of this wretchedness among the middle ranks since the peace account for the increased mortality among their children? The greater part of the deaths have arisen from diseases depending on general unsoundness of constitution.

The enormous mortality of the Orphan Hospital should lead to a strict inquiry into the physical management of the children, and salubrity of the situation where the building stands. An inquiry, in fact, was instituted in 1823, but we have not been able to learn whether it ever led to any precise conclusions as to the causes of the mortality. The situation is low; and in a quarter where the fogs which in spring and autumn infest the city are always peculiarly dense, and frequent.

life alluded to has been small, compared with what has been effected for earlier ages.

We come now to consider one of the most interesting sections in the work, that namely which treats of the relative mortality from different diseases at different periods of childhood. Few tables on this subject have hitherto been published, and none of these are both extensive and accurate. Mr Roberton has added to the stock one taken from the Register at the Rusholme-road cemetery in Manchester. The register, he informs us, is kept with unusual care, and the part chosen by him extends through a period of four years after April 1821, including upwards of 2000 deaths under the age of ten. It would lead to too great details were we to examine this table with the fulness which would be required for discovering the results it yields. We must content ourselves with showing how it establishes one of the leading objects of the author's work, the importance of paying increased attention to the physical education of children. Of the 2056 deaths it appears that no fewer than 994 originate in diseases more or less directly connected with errors in physical education, and particularly in disorders of the bowels. "No facts," says our author, "can show more forcibly the importance which ought to be attached to the physical management of children. Upon it chiefly depends, under all circumstances, the healthy condition of the digestive organs; and when it is neglected or conducted in error, the foundation is laid for many definite as well as anomalous ailments."

There are two other subjects connected with the mortality of children which Mr Roberton has investigated, the relative mortality of the sexes at different ages, and the relative mortality at different seasons of the year. Under both of these heads the reader will find some interesting discussion and information; but the conclusions at which he arrives are not very pointed.

The second part of the work treats of the physical education of children in regard to their health. This department of his subject is too concisely discussed to admit of a useful abridgement here. It consists of eleven sections,—1. On the structure, functions, and temperament of the body peculiar to infancy and childhood; 2. On the duties and qualifications of a wet nurse; 3. On the articles of aliment suitable for the early periods of life; 4. On the dieting of children in health and sickness; 5. On cleanliness, bathing, and the management of the skin; 6. On dress; 7. On air, temperature, and exposure; 8. On sleep; 9. On exercise and amusement; 10. On derangement of the alimentary canal, and on the management of the bowels; 11. On the hereditary transmission of peculiarities of structure, and the early management of such as are predisposed to particular diseases.

We have already observed that he appears to us to have given a very plain, judicious, and popular view of the physical management of children, avoiding equally the errors of those who attempt to insure the health of infancy by hardening the body, and of those who think to accomplish the same thing by overnursing. Persons concerned in the physical education of children, parents, and others, will do well to consult our author's opinions and directions.

In the following particulars we cannot coincide with him. In the *first* place, he discards too absolutely wine and malt liquors as articles of diet for children. Given regularly they will certainly in general prove injurious; but their occasional use has never appeared to us hurtful. Perhaps they do little good, given now and then to healthy children; but in laying down precepts on their physical education it is of some moment not to insist on what is matter of indifference,—in particular not to raise the voice against common practices, which are neither hurtful nor beneficial. *Secondly*, Mr Roberton is too indiscriminate in his abuse of foundling hospitals. Their utility or hurtfulness is assuredly relative: In this country they would undoubtedly prove injurious to morals; in some continental states it is to be feared that their abolition, without improving the morality, would greatly increase the misery of the lower orders. *Thirdly*, We do not exactly comprehend his instructions never to use coke or charcoal as fuel in nurseries. If he means that they ought never to be burnt in choffers and away from the chimney, the injunction is correct, and we believe called for. But that coke and charcoal should not be used at all as fuel, on account of "the celerity with which they yield carbonic acid," appears to us a vague statement. Wood and coal yield carbonic acid quite as quickly, besides other vapours also, such as sulphurous acid and empyreuma, which when diluted are fully more pernicious than dilute carbonic acid. *Lastly*, He is unnecessarily severe on the use of gas for lighting apartments. His reasons for condemning it are that, when unburnt, carburetted hydrogen escapes, which is very deleterious, as Sir H. Davy's experiments show; and that when burnt long in a close apartment, the air becomes very offensive. On this subject we can speak with confidence, having had some personal experience regarding it. We take it that no person would submit to breathe for any length of time an atmosphere sensibly impregnated with unconsumed gas; nor if he did, do we conceive that any obvious harm would result. It has never, ~~so~~ far as we have been able to learn, been remarked, that coal-miners suffer materially from breathing a dilute atmosphere of light carburetted hydrogen; gas-workmen, who constantly breathe a dilute atmosphere of the mixed carburetted hydrogen gases, do so with impunity; and we may add, that on the occasion of some experiments conducted along with a scientific friend, we spent the greater part of our working hours for three months in an atmosphere which all strangers felt disagreeable, without being able to observe any impression whatever on the health. As to the emanations from the consumed gas, these depend on the care with which the gas is manufactured and burnt. But good gas, burnt with ordinary care, although it does yield a little sulphurous acid, we know from the experience of several of our friends here, is very far from being the source of any annoyance in a dwelling-house.

We must close this analysis abruptly with expressing our sincere recommendation of Mr Roberton's work, and our expectation of seeing him ere long employ the talent he has shown for medical statistics on wider, more varied, and more definite materials. *

ART. V.—1. *Traité Theorique et Pratique des Maladies de la Peau, &c.—Treatise, Theoretical and Practical, on Diseases of the Skin; founded on new anatomical and pathological researches.* By P. RAYER, Titular Physician of the Dispensaries of the Philanthropic Society, Physician of the Central Board of Hospitals, &c. &c. Vol. I. Paris, 1826.

2. *A Practical Treatise on the Diseases of the Skin, arranged with a view to their constitutional causes and local character, &c. &c. 2d Edition, corrected and enlarged.* By SAMUEL PLUMBE, M. R. C. S. &c. London, 1827. Pp. 470.

PREVIOUS to the time of Willan our knowledge of cutaneous disorders, it will be generally admitted, was meagre, inaccurate, and unsatisfactory. The old work of Turner † contained, indeed, some good descriptive sketches, and here and there valuable practical remarks; and the *Exanthematologia* of Fuller ‡ furnished on the febrile eruptions such information as the opportunities of the time supplied.

But in neither of these works could it be said that the disorders incident to the skin were systematically arranged, or treated in a comprehensive manner. To say that the distinctions were inaccurate and deficient in precision is to mention the least of their blemishes. Wherever the authors deviated from the paths trodden by their predecessors, or attempted to introduce such new matter as casual observation supplied, the truth of nature was invariably disguised by shreds of the humoral pathology, or distorted by occasional reference to the mechanical theory of sound and morbid action.

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* For the table on the mortality from different diseases at different periods of childhood, see Medical Intelligence.

† A Treatise of Diseases incident to the Skin, in Two Parts, with an Appendix, concerning the efficacy of local remedies, and the manner of their operations. By Daniel Turner of the College of Physicians, London. 5th edit. London, 1736.

‡ Exanthematologia; or an attempt to give a rational account of Eruptive Fevers, especially of the Measles and Small-pox, &c. By Thomas Fuller, M. D. London, 1730.

Much the same character is applicable to a small Latin treatise first published in 1760, as the work of a most successful physician of Montpellier.* In this work, which we believe was ascribed to the celebrated Astruc, though professedly on cutaneous diseases, by far the greater part consists of loose, vague, and meagre descriptions of different forms of inflammation and their effects, of wens and ganglions, of aneurism and varix, with a few short remarks on itch, *herpes*, and leprosy, rheumatism and gout. It is clear, in short, that the learned author had utterly mistaken his subject.

The first treatise which was published with the pretensions of a complete system † was that of Lorry, to whom doubtless is due the merit of collecting most of the scattered information of his predecessors, and referring it to definite heads. The materials, though copious, and by no means destitute of information, are arranged, however, on a bad plan, or rather, are scarcely digested at all; and the want of nosological distinction and specific precision constitutes a serious defect in a tribe of diseases so multiplied and so mutually similar as those of the skin. Having imbibed the idea then current, that many cutaneous disorders or *eruptions* depended on some vice latent in the constitution, he adopted this as the character of those treated in the first part of his work; and these he again subdivided into *depuratory* disorders affecting the whole cutaneous surface, and depuratory affections confined to certain localities. The second part, consisting of diseases supposed to be primarily seated in the skin itself or its appendages, was also subdivided into two sections, upon the same principle, of the universal or local nature of the disorder.

Distinct as this arrangement appears at first sight to be, in following its several subdivisions and ramifications, the author was led into many inconsistencies and contradictions. Diseases known to belong to the cellular tissue were repeatedly introduced as examples of affections of the skin. One affection of the skin was often placed immediately contiguous to another of most different or even opposite characters. The same disease was not unfrequently found under two distinct heads, according as it appeared to be a depuratory eruption, or a disease primarily affecting the skin, and according as it affected this texture locally or generally.

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* Tractatus duo Pathologici, nunc primum in lucem editi. Auctore Medico Monspeliensi in praxi felicissimo, Tractatus Secundus, de Morbis Cutaneis. Amstelædami 1760.

† Tractatus de Morbis Cutaneis Parisiis, 1777. 4to.

These inconveniences were evidently in a great measure the result of the theoretical arrangement employed by the author. To supply this defect, the attempt made by Joseph Plenck was better calculated; but its merit consisted more in being the first and almost the only one, excepting those of the systematic nosologists, as Sauvages and Cullen, than in its intrinsic advantages. The first nine classes of Plenck's arrangement are not improperly made, and differ little, indeed, from that which was afterwards given by Willan. The tenth, including cutaneous ulcers, is manifestly referable to one or more of the previous nine; since every ulcer is the effect of some previous inflammatory disease of the skin. Wounds and injuries, which are placed in the eleventh class, cannot with propriety be admitted as a division of such a classification. The disorders of the nails and of the hair, which are placed in the thirteenth and fourteenth classes, are rather to be viewed as effects of certain morbid states of the textures and vessels concerned in their production, than as belonging to the same family with proper diseases of the cutaneous texture.

In the subordinate divisions, the defects of this arrangement were perhaps still more conspicuous. For not only was a state of the skin symptomatic of more general affection of the system not unfrequently converted into a disease with appropriate place and title; but various shades of the same disease were occasionally magnified into individual maladies. Even precise generic and specific distinction, the great object of all arrangement, was often neglected. Thus *gutta rosea* and shingles are referred to the head of *Maculæ rubræ*, along with erythema, measles, scarlet fever, and nettle-rash; *phyma*, a hard tubercular swelling, is placed with blebs and pemphigus, under the head of *Bullæ;* and goose-skin, a temporary state of the surface, tubercle, scurvy, leprosy, and elephantiasis, are all referred to the head of *Papulæ.** It would be difficult to perceive in what manner an arrangement of this description could be made subservient to precision in distinguishing the several forms of cutaneous eruptions. Admitting, even for the sake of argument, that the arrangement was as perfect as the subject then allowed, its manifest inapplicability as a guide to the practitioner is an insurmountable objection to its adoption.

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* Josephi Jacobi Plenck, Chirurgiæ Doctoris, &c. Doctrina de Morbis Cutaneis, qua hi morbi in suas classes, genera et species rediguntur. Editio 2do. Viennæ, 1783.

Such might be said to be the state of knowledge on the subject of cutaneous diseases, when Willan appeared. Unphilosophical arrangement, want of precision in the use of terms and definitions, and vagueness and confusion of description, may be truly said to be the difficulties with which he had to contend. That he has succeeded to a very great extent in removing them, is, we conceive, a point which few who have given the subject of cutaneous nosography any attention, will deny. By giving the terms then in use a fixed and determinate signification, by restoring such terms as correct observation showed had been used with accuracy and precision by the ancient physicians, by sound nosological distinction and accurate description, illustrated by useful historical and literary references, he introduced order where there was formerly only confusion ; he threw light on subjects previously involved in obscurity ; and he invested the subject of cutaneous nosology with a degree of interest which it never before possessed, and which at once raised it to a level with other departments of practical medicine.

In effecting this great and favourable revolution, Dr Willan was no doubt ably supported by his pupil and successor Dr Bateman. For to the latter belongs unquestionably the merit of completing the plan which Willan had only time to sketch and commence. The system thus completed has been generally admitted to be so free from serious errors, as to be adopted almost universally in every country where medicine is taught and practised. In Germany its superiority to that of Plenck has been admitted in the unequivocal testimony of Sprengel, Frank, and some of its most eminent physicians. In the Peninsula it has been made known through the treatise of M. Gomez.* But the greatest and most indisputable panegyric on the merits of the classification of Willan is afforded in the treatise of M. Rayer, who, though a physician of the same city which gave birth to the magnificent work of M. Alibert, not only adopts the nomenclature entirely, but even employs the arrangement with very slight modifications.

The classification of M. Rayer differs from that of Willan and Bateman, chiefly in presenting a much more extended, and doubtless a more philosophical view of the morbid processes, states, and actions incident to the cutaneous tissue. The arrangement of the latter authors contains principally those sorts of disease which consist in various forms either of inflammation, or deranged secretion, and their respective products, and one or two instances generally regarded as organic changes in the cutaneous tissue. M. Rayer, on the other hand, taking a view of the subject greatly more comprehensive, divides it at once into four principal sections. I. Diseases of the skin. II. Alterations of the appendages or productions dependent on the skin. III. Foreign bodies observed on its surface, or within its substance. And, IV. Diseases originally unconnected with the skin, but which communicate to it occasionally peculiar alterations. These four sections are subdivided in the following order.

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* B. A. Gomez ensoio dermosographico, o succincta e systematica descripçao das doenças cutaneas etc. con indicacoa dos respectivos remedios aconselhados in 4to. Lisboa, 1820.

Section I. Diseases of the skin.	Chapter I. Inflammations of the skin.	1. Exanthematous. 2. Bullar. 3. Vesicular. 4. Pustular. 5. Furuncular. 6. Papular. 7. Tubercular. 8. Scaly. 9. Linear. 10. Gangrenous. 11. Multiform.		
	Chapter II. Cutaneous and subcutaneous hemorrhages.	*Cyanosis*, *Vibices*, *Petechiæ*, *Purpura Hæmorrhagica*, *Ecchymosis*, *Dermatorrhagia.*		
	Chapter III. Neuroses of the skin.	Increase, diminution, abolition of cutaneous sensibility, without appreciable alteration of tissue.		
	Chapter IV. Changes in the colour of the skin.	Discolorations.	Leucopathie. Chlorosis.	1. Partial. 2. General.
		Accidental changes.	*Ephelis*, *lentigo*, *chloasma*, *meladermia*, *icterus*, *nævus maculosus*, bronzing from nitrate of silver.	
	Chapter V. Morbid secretions.	Epidrosis, spots or freckles, (*Tannes*,) follicular tumours.		
	Chapter VI. Errors in formation and texture; hypertrophy and accidental productions.	Anormal distension; scars, marks; *nævus hæmatodes*, subcutaneous vascular tumours; warts, pearly granulations; ichthyosis, horny excrescences.		
Section II. Changes in the dependances of the skin.	Chapter I. Changes in the nails and in the skin which produces them.	*Onyxis;* anormal growth of the nails; spots, coloured patches, decay, desquamation, reproduction of the nails.		
	Chapter II. Changes in the hair and in the generating follicles.	Inflammation of the bulbs; accidental changes of colour, hoariness; *alopecia;* scurfiness of the scalp, polish plait; accidental growth of hair.		
Section III. Foreign bodies observed at the surface, or in the substance.	Inanimate.	Fat; fat of the scalp of new born infants; inorganic substances, artificial colours.		
	Animated.	*Pediculus humanus; α pediculus capitis, β pediculus pubis; pulex irritans, pulex penetrans; acarus scabiei; oestrus, gordius.*		

SECTION IV. Diseases originating in other textures, but communicating occasionally peculiar changes to the skin.	Arabian Elephantiasis.

After this preliminary division into chapters or subsections, the first of these, or that which treats of the proper inflammatory affections of the skin, are subdivided in the following order.

Inflammations.	1. EXANTHEMATOUS.	Measles, rose-rash, scarlet-fever, nettle-rash, erythema, rose.
	2. BULLAR.	The vesication of blisters, *ampullæ*, pemphigus, *rupia*, zona.
	3. VESICULAR.	*Herpes*, itch, *eczema*, miliary eruption.
	4. PUSTULAR.	Chicken-pox, small-pox, cow-pock, spurious cow-pock, (*vaccinella*,) ecthyma, *gutta-rosea* (couperose,) chin-whelk, *(mentagra,)* moist tetter, *(impetigo,)* scall-head, and artificial pustules.
	5. FURUNCULAR.	Stye, boil, *anthrax*.
	6. PAPULAR.	*Strophulus*, *Lichen*, *prurigo*.
	7. TUBERCULAR.	Lupus, cancer, elephantiasis *Græcorum*.
	8. SCALY.	Leprosy, dry tetter, dandriff.
	9. LINEAR.	Chops and fissures, *(rhagades.)*
	10. GANGRENOUS.	Malignant pustule, carbuncle.
	11. MULTIFORM.	Scorch ; frost-bite ; syphilitic eruptions.

The arrangement of Willan, however, is not entirely exempt from objections ; and at different periods various criticisms on it have been offered both in this country and on the continent. In 1814 Mr Wilson published a familiar treatise on cutaneous diseases, in which he proposed to arrange them in four great divisions ; 1st, febrile cutaneous eruptions ; 2d, cutaneous inflammation without fever, but either simple or dependent on constitutional causes ; 3d, papular, vesicular, and scaly eruptions ; and, 4th, infantile eruptions, or those peculiar to infancy. It is easy to see that this arrangement, in which want of uniform principle is not the greatest blemish, is by no means suited for general adoption.

Some time afterwards an ingenious correspondent, Dr Marshall Hall, well aware of the weak points of the system of Willan, in imitation of a method often practised, but revived lately by Retz and Derien, proposed the plan of dividing all the cutaneous affections into three great orders, the acute or febrile, the chronic, and the local.

The boldest and least compromising innovator in this department, however, is Mr Plumbe, the second edition of whose work is now before us. Disapproving of the arrangement of Willan, first, because it overlooks the connection between cutaneous eruptions and the state of the constitution ; and, secondly, because it is too diffuse, and too much loaded with minute subdivision for practical utility, he conceives that a system more suitable to the views of the student and practitioner is found in the following method of arrangement.

Without wishing apparently to search for any uniform principle, or for any characters or set of characters, on the presence, absence, or modification of which, natural distinctions might be made, Mr Plumbe at once divides all the diseases incident to the human skin into six sections.

The first consists of "diseases which obtain their distinguishing characteristics from local peculiarities of the skin;" and to this head he refers the different forms of *acne*, or face-whelk; lupus, or *noli me tangere*; *sycosis*, or the chin-whelk; and the different forms of scall-head, or *porrigo*. Each of these genera, treated in a separate chapter, occupy the four chapters of the first section.

In the second section are placed "diseases marked by chronic inflammatory action of the vessels secreting the cuticle, producing morbid growth of the structure, and generally dependent on debility of system." To this head the author refers the Greek leprosy (*Lepra*,) the scaly tetter (*Psoriasis*,) and the dandriff (*Pityriasis*,) which are examined in the next three chapters.

The third section consists of "diseases exerting a probably salutary influence on the system originally produced by, and usually symptomatic of deranged digestive organs, and characterized by active inflammation." This is made to comprehend two species of scall-head *Porrigo favosa* and *P. larvalis* in one chapter; the papular eruptions incident to infants *(Strophulus,)* and adults *(Lichen,)* and *Prurigo* in a second chapter; and nettle-rash, the several forms of *herpes*, and the ordinary boil, in three separate chapters.

To the fourth section, including "diseases of a mixed character essentially dependent on inflammation, with which the constitution is not necessarily connected," are referred the moist or running tetter *(Impetigo,)* itch, *(Scabies,)* and the varieties of *Eczema*, in an equal number of chapters.

The fifth section is distinguished by containing "diseases dependent on debilitated and deranged states of the system, and consequent diminished tone of the vessels of the *cutis*." In this are considered the several forms of purple eruption, *(purpura,)* in one chapter, thrush *(aphtha)* in another; *Pemphigus* and *Pompholyx* in a third; *Ecthyma* and *Rupia* in a fourth; and *Erythema nodosum* in a fifth.

The sixth section, entitled "Fungoid diseases of the cutis and cuticle," consists of two chapters, in the first of which we find Ichthyosis and "Warts;" and in the second "concluding remarks," on the rose-rash, *(Roseola)* erythema, and venereal eruptions.

Such is the classification which Mr Plumbe proposes as a proper substitute for that of Willan and Bateman, which is represented as practically useless, and therefore to be abandoned. To criticise it, and show its imperfections would be easy; but this it is not our intention to do. It is farther manifest, that the author does not expect this; and therefore we say nothing more upon the subject, but leave it to the reader, who is as competent to the task as we are. Commend it we cannot; and we therefore trust he will excuse us from this duty also. We agree with him in thinking that the arrangement of Willan is defective, as probably every arrangement will be to the end of all nosology-making. We admit that it has faults, errors, and inconveniences, as who does not? All this we admit, and more perhaps, if it were a point of much moment to do so. But we are not prepared to adopt the opinions of Mr Plumbe, that the classification of Willan is practically useless, and that the present classification supplies all the defects, and amends all the errors of that of Willan and Bateman. We cannot perceive that Willan, had he lived, would have converted his system into such a monstrous production, as Mr Plumbe asserts, by voluminous details and expensive illustrations. We know well, as Mr Plumbe cannot fail to know, that the plan of Willan was as nearly as possible completed by Dr Bateman, both as to illustrations and descriptive details; and the synopsis of the latter author, which was completed on the plan originally sketched by Willan, may, we conceive, be safely adduced as a pledge, that no details into which these authors entered, would have been either voluminous, or of doubtful utility. In short, so far as we have hitherto observed, we cannot discover any good reason for discarding entirely the arrangement of Willan; and if we were disposed to make any preference, it would be in favour of that as modified by M. Rayer, which is perhaps more strictly pathological, and more in the spirit of good classification. Upon the whole, it might have been becoming in Mr Plumbe to show a little more indulgence to the failings of a system, which can perhaps be reproached with fewer faults than any yet presented to the profession.

Though we cannot commend the arrangement of Mr Plumbe, however, it is not our intention to assert that his work is in other respects deficient in merit. Several of the views on the etiology of cutaneous diseases, on which he undertakes to classify them, are not only highly ingenious, but give proofs of close and accurate observation. It is manifest that he has been very conversant with the various appearances assumed by cutaneous diseases; that he has studied their pathological characters with much attention, and often with success; and that he has devoted much of his time to improve the method of treatment. In short, though Mr Plumbe may not be the Linnæus of cutaneous nosography, he gives proof of understanding practically the subject of cutaneous diseases. We shall here take occasion to bring before the attention of the reader such few points in both works as may seem to merit regard; and in doing so, as we cannot follow the order of both, we adhere to that of M. Rayer as more suitable to our purpose.

Passing over entirely the exanthematous inflammations, in which, though there are many points of interest, yet none would justify detail in this place, we come at once to the *Bullæ* or blebs. In this order we find placed not only the bullar eruptions of Willan, but one of those which Bateman referred to the head of pustules. This is the *Rupia* or limpet-shell eruption, which M. Rayer justly remarks is formed rather from small blebs or vesications than from vesicles. Mr Plumbe agrees with Willan in placing it beside *ecthyma* as a pustular disease. There is a degree of difficulty in settling its precise character, insomuch as the disease is almost never seen in the commencement, unless in hospitals, where patients labouring under other diseases, especially the cachectic state, induced by long-continued syphilitic symptoms or mercurial disease, occasionally become affected with this eruption. After all we suspect, that though M. Rayer is not wrong in saying that the disease consists of vesications, these are symptomatic of a deeper affection of the corion; and that this approaches, as Bateman represents, to the character of a pustule deep set in the skin. It is certain that eventually the cutaneous substance is affected. The observations of Mr Plumbe on the circumstances under which this eruption appears are judicious, and have justly attracted the attention of M. Rayer.

The justice of another addition made to this order by transference from the vesicles we do not so clearly perceive. This is the form of eruption called by Bateman *Herpes zoster*, or shingles. We can say from personal observation that this eruption comes most properly under the head of the vesicles, that is, if a distinction between vesicles and blebs is at all practicable. M. Rayer, indeed, himself admits, that it consists of vesicles and blebs; and perhaps this might be the character of the eruption in the cases which he observed. Mr Plumbe agrees with Bateman in regarding it as a vesicular disease; and this must be at present admitted to be its true character.

Among the vesicular eruptions the first is *herpes*, in the history of which M. Rayer follows, with the single exception last mentioned, the steps of Dr Bateman. The description of Mr Plumbe, who connects its appearance with constitutional derangement, is, so far as we can perceive, accurate, and more accordant with the manner of viewing the disease to which we have been habituated. In distinguishing *herpes preputialis* from certain forms of venereal eruption, both authors notice the accurate observations of Mr Evans.

The history of eczema, or the red fret, a mercurial eruption, in the work of M. Rayer, is delivered in a very elaborate and accurate manner, and is illustrated by a selection of cases both from his own experience and from those of others. It is a remarkable proof of the greater attention with which the physicians of France at present study the works of foreigners than heretofore, that not only the writings of Pearson, Moriarty, and Alley on this subject are repeatedly referred to, but two of the mercurial eczema cases are given from Alley; and that by Dr Marcet of the eruption occurring spontaneously, is quoted from the Medico-Chirurgical Transactions. M. Rayer, however, makes no mention of the observations of Dr M'Mullin, Dr Spens, Dr Chisholm, or Dr Crawford. Several good cases of chronic *eczema* of the scalp, of the female breast, of the scrotum, of the hands, and other localities, are also adduced to illustrate the peculiarities of this eruption.

In the forms of pustular inflammation, the reader finds most confusion in both works. Thus M. Rayer refers to this head not only small-pox, ecthyma, impetigo, and porrigo, but varicella, and vaccinella, acne, and sycosis, *(mentagra.)* Without looking to their external eruptive character, Mr Plumbe, as may be seen from his classification, assembles under the same head not only *lupus*, *acne*, and *sycosis*, but three of the forms of *porrigo.*

These eruptions, Mr Plumbe shows, are more nearly allied than at first sight they appear to be. Acne and sycosis, he conceives, depend on obstruction of the sebaceous follicles. On the origin of lupus he speaks less clearly. And the species of *porrigo* termed *furfurans*, *lupinosa*, and *scutulata* he endeavours to trace to a source not dissimilar.

In this theory there is some plausibility. The form of *acne* called *punctata* depends doubtless on a morbid state of the cutaneous follicles; and perhaps the ordinary whelk (*acne simplex*) is the simplest form of this disorder. " In its simple and most trifling form," says Mr Plumbe, " the disease consists merely of obstruction of the sebaceous follicles, in consequence of their contents becoming too hard to pass readily to the surface. Inflammation of the follicle, and the production of what is called a pimple results, and is soon followed by the formation of matter; the follicle is destroyed by this process; the matter is discharged; a little redness remains for a day or two, and the part returns to the healthy state." The black points Mr Plumbe justly represents as nothing more than the blackened surface of the contents of the inflamed follicles. The indurated and rosaceous species he regards as the simple, allowed to proceed so far, and recur so often, that it effects a change in the structure of the surrounding skin. The subsequent description shows that Mr Plumbe has studied this disease with much attention, and upon the whole with success.

The constitutional disposition favourable to this disorder is represented as allied to plethora; a state of the system he conceives which must lead to a more solid and adhesive secretion of the sebaceous follicles. In the cases of *acne indurata* and *rosacea*, he regards it as connected with habitual disorder of the digestive organs, occasionally connected with strumous diathesis.

Disapproving entirely of the ordinary mode of treating this affection, as totally inconsistent with good surgical principles, Mr Plumbe recommends inflammation to be soothed, and suppuration promoted by poultices. After this, he follows Bateman in recommending spirituous lotions; and of these he gives the preference to an embrocation consisting of corrosive sublimate, dissolved in proof spirit, in the proportion of five grains of the former to eight ounces of the latter.

The application of the fact of obstructed follicles to explain the origin of atheromatous tumours, is erroneously ascribed to Sir Astley Cooper. That eminent surgeon certainly describes in his essays encysted tumours formed in this manner; and we sincerely believe that he did not derive this opinion from any other person, and that the theory in relation to him is original. The same circumstance was observed, however, by Plenck * in his system of tumours, and is repeatedly and distinctly mentioned by Monteggia as a fact well established. † Sir Astley Cooper, nevertheless, has the merit of directing the attention of English surgeons to this as a cause of follicular tumours; but it is requisite to remember, that the explanation applies to one species only of encysted or folliculated tumour,—that occurring in the substance of the skin.

The disease named *sycosis*, or the eruption of whelks on the chin and beard, Mr Plumbe contends, is nothing more than *acne* or follicular obstruction and its consequences occurring in parts covered by hair. He therefore does not admit the propriety of the distinctions into chin-whelk and scalp-whelk. His explanation of the mechanism, as it may be termed, of this disorder deserves perusal.

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* "Eadem mala contingunt si glandulæ subcutaneæ porus excretorius diu obduratus sit." Systema Tumorum Josephi Jacobi Plenck, Classis VII., p. 151.

† Istituzione Chirurgiche, Volume II.

"The part occupied by the beard is generally pretty well supplied with sebaceous follicles, and these are of course equally liable to disorder with those in other parts of the skin, from constitutional causes. When, however, any accidental circumstance brings on inflammation and disorder in them, the peculiarity of their situation abounds with impediments to its termination in the most desirable manner. The mouth of the inflamed follicle, and the adjacent *cutis*, is penetrated by hair; and the violence inflicted by frequent shaving makes every individual hair a powerful mean of adding to the mischief. If a disposition in the inflammation of the follicle to subside may exist, the influence of the operation of shaving is sufficiently great to prevent it, and to hurry on the inflammatory action to the suppurative process. If many tubercles are formed, it is capable of increasing their size, and extending the inflammation to the adjoining follicles."—"From the local influence of the hair on the inflamed spot, where it penetrates the *cutis* alluded to, a secretion of pus is formed around many of them, the hair being situate in its centre; and these pustules, intermixed with the tubercles formed by the inflamed follicles, some of which are also showing matter on their *apices,* make up the external characters of the disease. The same phenomena are also observed when it occurs in the scalp; the extraction of single hairs here and there forming orifices by which the contents of a pustule are readily discharged. As in common cases, if a lancet be thrust into the tubercles of sycosis, which is advisable where one or more hairs are not distinctly seen growing in it, matter will certainly be found, and a rapid subsidence of the inflammation will follow. Where hairs can be pulled out in the way mentioned, however, the evacuation of the matter is accomplished by gently squeezing the tubercle, without having recourse to the lancet."

These views, if not altogether novel, are certainly both ingenious and natural, and unquestionably deserve the attention of the practitioner. Little doubt can be entertained that the bulbs of the hair are affected in these forms of cutaneous disorder; and it is perhaps of no great moment to the question, whether their affection is a cause or an effect of the malady. The observations of Mr Plumbe are in either case applicable.

In the application of the same views to the scalled head, *(Porrigo)* Mr Plumbe contends for abolishing the numerous forms into which this disease has been distinguished according to the kind of discharge; and, discarding these circumstances as accidental, he undertakes to trace it at once from its first appearance to its established form. In this he finds it requisite to abandon entirely the distinctions employed by Bateman and Alibert.

According to the observation of Mr Plumbe, the first form of the disorder, or that which is most generally found to commence the others, is that in which, according to Turner, "the hair falls off not altogether from the root, but piecemeal." Along with this the skin of the scalp becomes rough and scurfy in patches, the margin of which is distinctly marked, while the centre is particularly scurfy. It is only when stimulating substances are applied, according to Mr Plumbe, that the minute pustules, called *achores*, are formed on the margins and centre of the patches. He expresses himself, nevertheless, as if the patches were originally formed by minute vesicles, scarcely discernible to the naked eye, and rarely seen unbroken; and as if their conversion into *achores* depended either on the constitution of the individual, or on the mode of treatment. This applies especially to the proper ring-worm of the scalp, or that in which the patches assume a circinate or circular form.

In the only other form admitted by Mr Plumbe, he represents the disease as consisting of pustules never assuming the circular disposition, but dispersed diffusely over the scalp, and *every individual pustule having a hair growing through its centre;* the intervals of the scalp being red and inflamed. This diffuse or pustular form of the disease, which is not attended with immediate loss of hair, he states to be found chiefly among children of dark hair and unhealthy constitutions.

To the treatment of the first form, in addition to the ordinary practices of ablution, &c. Mr Plumbe applies lightly the concentrated sulphuric acid to the affected parts of the scalp by means of a feather. This is followed by slight redness of the surface and some smarting, when the residual acid is removed by a sponge dipped in warm water. By this application the cuticle is destroyed and the vessels of the corion are excited; and in the course of a few hours they secrete a copious exudation of lymph, which forms a scab of variable thickness. In a few days this dries and is thrown off, with the residual unhealthy hairs. The spot exposed is bald, red, and tender; but in a short time the cutaneous surface recovers its natural properties, secretes healthy cuticle, and produces natural hair.

In the treatment of the diffuse pustular form, after removing the hair by finely-pointed scissors, and cleansing the scalp by frequent ablution with water as hot as can be borne, he regulates the diet and general health of the patient. After a time he recommends the internal administration of corrosive sublimate, in doses of from one-eighth to one-fourth of a grain in tincture of bark, three or four times daily, according to the age of the child. A few grains of *hydrargyrus cum creta*, with an adequate quantity of rhubarb, are also recommended to be given about twice a week.

It is no slight objection to the arrangement of this author, that, for the history and treatment of two species of this disease, *porrigo favosa*, and *porrigo larvalis*, the reader is compelled to turn to section third. This shows that the author is at least sincere, if not consistent, in his principles; and that he does not modify diseases to suit the peculiarities of his arrangement. The description of these forms of the complaint is derived chiefly from Bateman and Alibert. In the treatment of the favous species, a solution of nitrate of silver, or sulphate of copper, is recommended as an effectual application. In both he inculcates repeatedly the necessity of attending to the state of the general health, and of correcting all disorders dependent on constitutional causes.

On lupus, which Mr Plumbe labours to refer to the same head, his opinions are less precise, or less clearly stated. Though its occurrence on the nose might be ascribed to follicular obstruction and inflammation, it is manifest that its appearance in other situations where follicles are wanting, is an objection rather strong to the justice of the theory. For the latter purpose he is obliged to admit, as others, the existence of a tubercle in the cutaneous tissue. The history and description of the disease is not very systematic. But it contains some good practical remarks.

The foregoing observations may communicate some idea of the nature and objects of the work of Mr Plumbe. Though we do not think it is likely to supersede the synopsis of Bateman, which must continue to be read as long as good description and distinct arrangement are valued, it nevertheless contains much practical information, and is indispensably necessary to every one who would understand the treatment of cutaneous diseases. It is illustrated by two good coloured engravings.

On these subjects the treatise of M. Rayer, which, upon the whole, is composed on the model of that of Dr Bateman, does not present much to merit especial notice. The subjects are examined systematically, and in proper order. The descriptions are clear and accurate, and contain all the usual information. We cannot perceive, however, that on the doubtful points he has thrown much additional light. His cases are valuable practical commentaries on the history and treatment of the different cutaneous disorders; and though they much increase the size of the work, they contribute greatly to illustrate the circumstances peculiar to each variety of cutaneous disorder.

In the present volume, the author does not proceed beyond the tubercular eruptions; and the four last subdivisions of the first chapter may therefore be expected to commence the second volume. The plates, though rather coarse, are valuable representations of the objects, and will materially aid the student in studying the peculiarities of different skin diseases. It is evident that they form a most useful substitute for the expensive engravings of Alibert, and Willan, and Bateman, which can be attainable to very few only.

ART. VI.—*A Practical Essay on Stricture of the Rectum, illustrated by cases, showing the connection of that disease with affections of the urinary organs and the uterus, with piles, and various constitutional complaints.* By FREDERICK SALMON, Surgeon to the General Dispensary, Aldersgate Street, &c. London, 1823. 8vo. pp. 188.

THE object of Mr Salmon's work is to call the attention of practitioners to a disease which he thinks is too much neglected, and too often overlooked; to prove that stricture of the rectum is of much more frequent occurrence than is generally imagined; to explain its pathology; to lay down the most approved treatment, medical and surgical; and to illustrate by an ample collection of cases its essential symptoms, and the collateral affections of which it is more or less directly the cause.

The treatise is founded almost entirely on the results of personal experience. The inquiries of preceding authors have indeed been rather too much disregarded; for among the authorities referred to, we do not so much as find the name of Mr Calvert, the latest writer on the subject, or that of any of the foreign surgeons who have contributed their share to our knowledge of the disease. But to counterbalance that defect, Mr Salmon's book contains many original observations and practical hints, which the practitioner will be able to turn to good account; and the details of some of his cases in particular are very valuable.

If the cases he has related are all genuine examples of stricture of the rectum, which there seems no sound reason for doubting, the frequency of the disease is sufficiently established by the author's catalogue. For of the twenty-eight cases which he reports, no fewer than twenty-two have occurred within the last four years,—no inconsiderable number to fall under the notice of a single surgeon, and one too of short standing in the profession. Making every possible allowance for cases being liable to escape observation on account of errors in diagnosis, we still must come to the conclusion, that stricture of the rectum is unusually frequent in London. In our own city at least it is in comparison exceedingly rare.

Mr Salmon commences with a short account of the anatomy and physiology of the rectum. He considers it as composed of three portions; the first or upper portion extending from the termination of the sigmoid flexure of the colon half way to the anus, and having its concavity forwards; the second or middle portion extending to the prostate gland, and having its concavity upwards; the third, comprehending the last two inches of its course, and having its concavity turned slightly backwards. This statement is necessary for explaining the cause of the seat of stricture; which in fact occurs always at one of the junctions of the component curves of the rectum with one another. Its most frequent seat, according to our author, is at the junction of the upper and middle curves, five or six inches from the anus, *and consequently beyond the reach of the finger.* The next most frequent seat is where the sigmoid flexure of the colon terminates at the rectum. And it is least frequently found, —where chiefly or solely it is looked for in suspected cases,—a short distance above the *sphincter ani.*

In next treating of the morbid anatomy and causes of the disease, he passes cursorily over carcinomatous stricture, which occurs chiefly near the verge of the anus, stricture from enlarged prostate gland or uterus, or from excrescences of the mucous membrane of the gut; and examines in greater detail what he considers by far the most frequent variety, which is analogous, if not identical in its pathology with stricture of the urethra, and consists in the first instance of spasmodic contraction, and afterwards of thickening and induration formed by an effusion between the muscular and villous coats. This variety originates in constitutional narrowness of the sigmoid flexure of the colon—unusual prominence of the superior part of the *os sacrum*—the irritation caused by drastic purgatives—or too great power or spasm of the *sphincter ani*, necessitating violent efforts for the evacuation of the gut. Mr Salmon denies that it is ever produced by syphilis.

Under the next head, which contains a short exposition of the incipient symptoms of stricture, there is nothing new, or which calls for special notice. These symptoms are exceedingly obscure, so that the disease often attains an advanced stage before its existence is suspected. They only serve to rouse the suspicion of the practitioner, and direct him to the necessary examination of the rectum.

The next topic, the treatment, gives rise to some observations on the constitutional management of stricture, in which in particular he condemns the free employment of laxatives administered by the mouth, and lavishes his encomiums on the habitual use of injections,—not only, indeed, in such cases, but likewise in habitual constipation generally. But the author's chief attention is turned to the local treatment, under which he considers the bougie, and its proper material and form; the advantages of topical remedies applied through the medium of the bougie; and the propriety of the operation of dividing the stricture.

The bougies he employs are eleven inches long; the largest is three inches and a half in circumference, the smallest one inch; and between these he has ten others of various sizes. They are made of "fine linen, heavily coated with wax and a certain portion of diachylon plaster, mixed with a small quantity of lamp black;"—a composition which he says becomes sufficiently soft and pliable by immersion in hot water, and afterwards, when in the bowel, hardens sufficiently to afford considerable resistance to the stricture. A tape with a loop at the lower end passes along the whole length of the instrument. He condemns all elastic gum bougies, and still more those made of metal. The manner of introducing them we give in the author's own words.

"A full-sized bougie, not less than eleven inches in length, thoroughly softened and well oiled, adapted to the shape of the passage through which it is to be passed, is to be introduced with the convexity of its first curve towards the sacrum, in which way it is to be passed upwards and backwards about two inches through the third portion of the bowel, provided it gives no pain; for the introduction will commonly produce an uneasy sensation. We continue to propel the bougie in the same direction, about three or three-and-a-half inches higher, or through the second portion of the rectum. The point of the instrument will now bear directly upon the hollow of the sacrum, and the but end towards the left side of the body. With the view, therefore, of avoiding the sacrum, and of accommodating the instrument to the great curve of the rectum, we change its position, by describing the segment of a circle from left to right, with the but end, turning it upwards, at the same time continuing to propel the instrument. Having described this segment, we shall have carried the bougie full four inches farther, or to what may be considered the extent of the rectum. But it is yet to be introduced into the sigmoid flexure: we therefore slightly depress the but end of the instrument, at the same time propelling it upwards till the whole is fairly within the sphincter.

"Upon encountering obstruction, trifling pressure is to be maintained for a minute or two, and if under this careful pressure the pain increase, and the instrument remain stationary, it is to be withdrawn, the next size introduced, and so on from above downwards, till we ascertain the size which passes with trifling pain or difficulty fairly into the sigmoid flexure." (p. 41.) The instrument, retained by a tape through the loop, is then to be kept in the stricture for ten or fifteen minutes or longer, according to the ease with which it is borne. It ought not to be introduced above once every third day. In this respect our author's practice differs from that of others who have treated of stricture, most of them recommending the introduction of the bougie daily. Mr Salmon maintains that the advantage derived from dilatation of the passage is counterbalanced in that case by the frequency of the irritation; sees no good reason for using the bougie oftener in stricture of the rectum than in stricture of the urethra; and informs us that, since he has ceased to use the bougie daily, and made it his practice to introduce it only once every three or five days, he has effected the required dilatation more rapidly.

The subject of medicated bougies is discussed in half a page. Mr Salmon trusts entirely to the mechanical power of the instrument, doubts the efficacy of medications applied through its intervention, and confines the employment of them in practice to the besmearing of the bougie with an opiate ointment when the local irritation is too great.

The introduction of the bougie, he adds, very generally causes pain over the whole abdomen, and likewise, at first particularly, a distressing desire to evacuate the gut. Occasionally cramps of the limbs follow its use, sometimes numbness, sometimes rigors, sometimes sickness. The abdominal pains are at times so violent as to make an inexperienced person apprehend inflammation. If they continue any considerable length of time after the instrument has been withdrawn, an aperient will remove them. The greatest annoyance during the presence of the instrument in the bowel arises from the irritation and contraction of the sphincter muscle. This is best prevented by pushing the entire bougie beyond the stricture, a proper tape being attached to the loop of the instrument, in order to withdraw it when necessary.

Having concluded the method of treatment by the bougie, the author proceeds to explain the operation for division of the stricture. His illustrations are exceedingly short. He utterly condemns, in fact, the use of the knife in stricture of the rectum, except when the stricture is so near as to be reached by the finger, which is rarely possible. Even then he has seen the operation followed by peritoneal inflammation.

A few cursory remarks then follow on carcinoma of the rectum, its distinction from proper stricture, and the variations required in the treatment of obstruction of the gut when dependent on this disease.

The remaining and indeed the larger part of the book consists of a series of cases illustrating the relations of stricture to other affections of the intestinal canal and adjoining organs,—affections which often spring from obstructed rectum, though on a careless examination they would appear to be primary diseases.

The first disorders of the kind are affections of the urinary organs. Nine cases are described which exemplify the connection of these disorders with obstructed rectum. Mr Salmon infers from his observations, that stricture of the urethra, frequent micturition, retention of urine, hæmaturia, diseased prostate, as well as neuralgic pains, and even inflammation of the testicle may be produced, or at least kept up by stricture of the rectum. In some of the cases of stricture of the urethra, we suspect it may be questioned whether the obstruction of the rectum was not the secondary rather than the primary disease. We must refer for the particulars to the work itself, as the cases are hardly susceptible of abridgement.

The next class of diseases, whose connection with stricture of the rectum is examined, comprehends affections of the uterus and vagina. Under this head five histories are given. The first is an example of enlarged uterus attended with dysmenorrhœa, discharge from the vagina, dysuria, and tenderness of the os tincæ; all of which symptoms, except the enlargement of the uterus, disappeared when a stricture in the rectum was reduced by the bougie. The second is a case not of stricture but of habitual obstinate constipation, illustrating by analogy the effects of stricture. It had caused three miscarriages, and excited constant dysuria and other annoyances; but, being removed by the regular use of injections, the lady carried her child to the full time, and got rid of all her local complaints. The third is similar to the first, with the addition, that miscarriage occurred several times, and the disease of the rectum had advanced to ulceration, and general induration of the gut. The bougie was not used. The fourth is an instance of frequent miscarriage, fluor albus, dysuria, flooding, connected with a warty and ulcerated state of the mucous coat of the rectum, and stricture at the sigmoid flexure of the colon. Partial relief was derived from the bougies, which, however, the patient declined continuing; and she died a few weeks after of apoplexy. The fifth is likewise an example of frequent miscarriage, with numerous ailments connected with the uterine and urinary organs, and arising from constipation, the result of stricture. A cure was effected by the regular use of the bougie.

The next affection originating in stricture which Mr Salmon considers is enlargement of the colon. Two striking cases of this description are related, one of them fatal, the other a case of recovery. We shall give an abstract of the former, as it is important in many of its features, and illustrates admirably the effect of stricture in disordering the alimentary functions and organs, without calling the attention of the physician to the primary seat of disease by any prominent symptom. A middle aged gentleman was affected with severe dyspepsia, ulcerated sore throat, and neuralgic pains in the limbs, which were rashly imputed to syphilis, and violently treated by three mercurial courses. The bowels were constantly disposed to be torpid, with occasional intervals of diarrhœa and hæmorrhoïs. By regulating his diet and attending to the state of his bowels his complaints were mitigated; but they returned whenever he was guilty of any irregularity, to which he was naturally much inclined. Subsequently he was attacked with symptoms of pleurisy, which was checked by cupping, but was followed by dropsy of the limbs, and not long after by enlargement and tenderness of the abdomen, without distinct fluctuation. He had now also a constant desire to pass water and go to stool; but the calls were for the most part ineffectual. Subsequently fluctuation could be felt in the abdomen; the upper part of it was excessively tender; a hardness and fulness could be felt in the right hypochondrium; and in consequence the disease was considered dropsy, depending on enlarged liver. At length matters seemed to be drawing towards a close; he became insensible, the pulse intermitted, and brandy was required to rally the sinking powers of life; when unexpectedly he was attacked with profuse diarrhœa and considerable hæmorrhois, from which he experienced great relief. Mr Salmon then began to suspect the existence of stricture of the rectum, and on examining the gut found that about three inches from the anus there was an obstruction which prevented the passage of a larger instrument than a full-sized urethra bougie, and that beyond that point the instrument passed through feces. A small quantity of an emollient injection, introduced by means of an elastic catheter, brought away an enormous quantity of fetid feces. This was repeated with the same effect six successive evenings. The abdomen was then very much diminished in size, all appearance of enlarged liver had vanished, the swelling of the legs had abated, all his sufferings were lessened, and his general health much improved. The rectum was now carefully explored; and it was found that the sigmoid flexure would barely pass a full-sized urethra bougie, and indented it deeply, just like a stricture of the urethra. Some days afterwards the smallest rectum bougie was passed and retained two hours; the size was then gradually increased, till in 33 days Mr Salmon's seventh number could be passed. All this while the abdominal swelling and tenderness, the anasarca, dyspepsia, and all his other ailments went on mending progressively, so that at length he was able to walk to his surgeon's house, a distance of three miles. Mr Salmon then lost sight of him for a month, during which he relapsed into his irregular habits, and soon became almost as ill as ever. He resumed the treatment, but subsequently abandoned it in disgust, when he found his patient one day in a state of intoxication. Six months afterwards he was sent for, and found him rapidly sinking under all his old complaints; frequent vomiting ensued; and at length he died in a state of stupor from gradual exhaustion. On examining the body, it was found that the colon was so enormously distended as to occupy the whole anterior part of the abdomen, the transverse arch reaching from the *epigastrium* to the *symphysis pubis*, and the ascending and descending portions nearly coalescing. The gut was connected along its course by adhesions to the liver, diaphragm, stomach, and abdominal parietes; and it was filled with feces. The sigmoid flexure was so contracted, *that even water would not pass through.* The whole colon was much thickened, apparently from a deposition between the mucous and muscular coats; and its mucous coat was inflamed, and here and there ulcerated. Besides the stricture of the sigmoid flexure, there was another in the rectum seven inches from the anus, and a third three inches lower down. The small intestines were agglutinated together. The liver was rather hard, and small, but not particularly diseased; the other abdominal organs, as well as those in the chest, were healthy.—(Pp. 122-140.)

The other case, of the same nature with the foregoing, was characterized by enlargement confined to both sides of the abdomen, hard and painful on pressure, and varying in size according as relief of the bowels was obtained by attacks of purging, which alternated with obstinate constipation. His experience in the former case led Mr Salmon to suspect stricture, and accordingly, after the bowels had been cleared of an enormous accumulation of feces by repeated injections, he found on examination a stricture at the sigmoid flexure, which admitted only the smallest rectum bougie. The instrument came away besmeared with the discharge of an ulcer. Nineteen days after the treatment was commenced, number seven of the bougies could be introduced. "The improved appearance of the patient was surprising; he had entirely lost his cough, difficulty of breathing, and night sweats; his pulse was steady and good at seventy; he slept comfortably in any position; the irritability of the bladder had completely subsided; his motions were healthy; the enlargement of the abdomen was nearly gone, his size round that part being diminished by measure eight inches and a-half." The bougie no longer came away smeared with discharge. Three weeks afterwards number nine was passed; he had no particular complaint, and all that remained of the swelling was a puffy enlargement in each groin, about the size of a hen's egg. He relapsed, however, in consequence of getting into irregular habits; but got better again by attending to the state of his bowels and living more regularly.

The details of these two cases are full of interest, and render them the most valuable in the whole work.

The next disorder which claims a share of Mr Salmon's attention as connected with stricture is piles. Seven cases of this disease are briefly described, in which there was a concurrence of stricture, and in which the removal or diminution of the stricture put an end to all the direct and collateral inconvenience caused by the piles. The tendency of stricture to excite piles, and to increase the irritation arising from them is manifest.

The last subject which engages the author's inquiries is the effect of stricture of the rectum in exciting determination of blood to the head, with its usual concomitants and sequelæ. It is needless to quote any of his illustrations of this point. The effect is produced through the medium of habitual constipation, which is well known to be a common cause of the disorder in question. Mr Salmon has related four cases of habitual and obstinate headach attended with giddiness, and sometimes dimness of vision, which he traced to constipation resulting from stricture of the rectum, and which were more or less completely relieved, according as he was able to palliate or remove the origin of the evil.

The volume concludes with these remarks.—The foregoing analysis will satisfy the reader that it contains a good deal of practical information on a topic of considerable obscurity. We have only one advice to give Mr Salmon at parting,—namely, to abandon, the next time he appears before the public, a strange practice he has fallen into of interspersing scraps of mongrel and often false Latin throughout his English narrative. They are quite unnecessary, for aught we can discover; and are tolerated by no principle in correct writing with which we are acquainted. What useful purpose can be served by such sentences as these? "Ordered absolute rest, *hirudines decem*, and a cold lotion to the part, *oleum ricini omn. alt. mane*, and a saline aperient occasionally."—"I advised a very mild injection *ter in die*, local fomentations, with *oleum ricini pro re nata*."

Art. VII.—*A Treatise on the Diseases of the Chest and on Mediate Auscultation.* By R. T. H. Laennec, M. D. &c. 2d Edition, greatly enlarged. Translated from the French, with Notes, and a Sketch of the Author's Life. By John Forbes, M. D. &c. With Plates. London, 1827. Pp. 722.

THE work of M. Laennec has on two separate occasions occupied a large share of our attention. Upon the last, when the original of the second edition came under examination, we completed what we conceived to be requisite in the sketch of the contents of both ; and we entered as fully as the nature of these pages admits into the material contents of the work.

The present volume is a complete translation of that edition, and consequently comprises every addition and change which the lamented author introduced, previous to the approach of the disease which terminated his useful life. Though the circumstances now mentioned render it quite superfluous to enter minutely into the contents of the work, it is nevertheless requisite shortly to apprise the English reader of the obligations under which he lies to Dr Forbes.

The duty of translation, though one of very great utility, has been accounted so humble in its nature, as rarely to have attracted the attention of those, whose accomplishments might seem adequate to an original work. It has hence too often happened that translations have been executed by persons who, however competent to the task as scholars, were but imperfectly acquainted with the subject of the work to be translated. From this evil it is well known books of science have not at all times been exempt; and perhaps to this cause partly is to be ascribed the contempt with which many look on translations.

We rejoice to perceive that a better era has commenced ; and that translations are now executed with as much zeal, sincerity, and competency, as if the reputation of the translator himself depended on it as an original work.

And, in the first place, understanding that a translation of the work of M. Laennec is indispensable to diffuse extensively among the profession in this country, the knowledge of the pathological researches of that physician, it is almost unnecessary to say, that the task could not have fallen into better hands than those of Dr Forbes. By his translation of the first edition, indeed, this gentleman proved himself most worthy to communicate to his countrymen the knowledge of the inquiries of M. Laennec. Already possessed of considerable knowledge of the subject of diseases of the heart and lungs, and zealously devoted to their further cultivation, he has since that spared no pains either to verify, to extend, or to correct the labours of his author. Dr Forbes, it may be added, has devoted himself with particular attention to the verification of the diagnostic methods employed with so much success by M. Laennec.

It is easy to perceive, that, by one possessed of such qualifications, a work like that of M. Laennec had every chance of being faithfully and properly translated. We are happy to find that this expectation is not disappointed. The translation is clear, faithful, and accurate throughout. The language is plain, unassuming, and sufficiently good for the purpose. And Dr Forbes has undertaken and completed his task like one who knows not only the language but the subject of his author. He has not confined himself to the mere task of slavish translation. He has exercised his discretion in omitting such parts as are either mere refinements, or points which, though curious, possess little or no practical utility.

On the other hand, Dr Forbes has increased, in no ordinary degree, the value of the work of M. Laennec, by various additions in the form of notes. We never attempted to conceal the fact, that the work of Laennec, though the best of modern times, is not entirely faultless. Had it been so in relation to France, it was still likely to require modification to suit the English physician.

Some of his pathological views might, like those of every founder of a system, be perhaps too much generalised or carried to an extreme length. Others again might be deficient for want of illustration and confirmation. And in others, perhaps, M. Laennec, like many French writers, might betray less acquaintance with the works of foreigners than is requisite for the author of a complete treatise on any branch of pathology.

With the view of remedying evils of each description, Dr Forbes has been at great pains to modify, confirm, or amend several of the results of his author by information collected either from personal experience, or from that of his predecessors or contemporaries. This information, introduced as it is in the form of notes, while it does not interrupt the train of the author's discourse, is of great value to the reader, by showing him how far the conclusions of M. Laennec are confirmed by the inquiries of others, and how far they confirm those of previous inquirers. These notes, especially those which illustrate the literature of several of the subjects, are particularly useful, as they show the reader, at a single glance, the principal sources of information on any given subject. Of this kind the notes on the chapter on the various forms of catarrh, on croup, on asthma, on pulmonary apoplexy, on pneumothorax, and on various diseases of the heart, will be read with much interest. In short, it is impossible to specify one more than another; for Dr Forbes has collected, from the most authentic sources, whatever information might illustrate the subject.

The therapeutical department, however, is probably the least perfect of the work of M. Laennec. Several of its weak points must be conspicuous to the English physician, who has been long accustomed to a much more energetic style of practice than is employed by his brethren on the continent; and they are particularly so in the second edition, in which the ingenious author introduced much more therapeutical information than in his first. On these topics Dr Forbes has occasionally made some good remarks, and introduced some useful information. Of this kind are the observations on the treatment of peripneumony, especially the *English practice*, as it is emphatically named among foreigners, of copious blood-letting, and on the efficacy of calomel and opium. The observations on the influence of climate on consumption are highly interesting, and derive additional value from being the result of the observation of Dr James Clark, who, it is well known, has made this the subject of especial inquiry.

It was not our intention to make any extracts from this work, as it is impossible in this manner to do it justice. The following, however, appears to possess so much interest, as a curious illustration of the etiology of pulmonary disease, that, coming as it does from M. Laennec, while it communicates useful information, it may be taken as a good specimen of the general merit of the translation.

"Among the occasional causes of phthisis, I know none of more assured operation than the depressing passions, particularly if strong and of long continuance; and it is worthy of remark that it is the same cause which seems to contribute most to the developement of cancers, and all the other accidental productions which are not analogous to any of the natural tissues. This is perhaps the only cause of the greater frequency of consumption in large cities. In these, the single circumstance of the inhabitants having more numerous relations with one another, is in itself a cause of more frequent and deeper vexation; while the greater prevalence of immorality of every kind, is a constant source of disappointment and misery, which no kind of consolation and not even time itself, can effectually remove. I had under my own eyes, during a period of ten years, a striking example of the effect of the depressing passions in producing phthisis, in the case of a religious association of women, of recent foundation, and which never obtained from the ecclesiastical authorities any other then a provisional toleration, on account of the extreme severity of its rules. The diet of these persons was certainly very austere, yet it was by no means beyond what nature could bear. But the ascetic spirit which regulated their minds was such as to give rise to consequences no less serious than surprising. Not only was the attention of these women habitually fixed on the most terrible truths of religion, but it was the constant practice to try them by every kind of contrariety and opposition, in order to bring them, as soon as possible, to an entire renouncement of their own proper will. The consequences of this discipline were the same in all: after being one or two months in the establishment, the catamenia became suppressed; and in the course of one or two months thereafter phthisis declared itself. As no vow was taken in this society, I endeavoured to prevail upon the patients to leave the house as soon as the consumptive symptoms began to appear; and almost all those who followed my advice were cured, although several of them exhibited well-marked indications of the disease. During the ten years that I was physician of this association, I witnessed its entire renovation two or three different times, owing to the successive loss of all its members, with the exception of a small number, consisting chiefly of the superior, the grate-keeper, and the sisters who had charge of the garden, kitchen, and infirmary. It will be observed that these individuals were those who had the most constant distractions from their religious tasks, and that they also went out pretty often into the city, on business connected with the establishment. In like manner, in other situations, it has appeared to me that almost all those who became phthisical, without being constitutionally predisposed to the disease, might attribute the origin of their complaint to grief, either very deep or of long continuance. Severe continued or intermittent fevers would seem to be pretty often the occasional cause of the production of tubercles ; since it is not unusual to find, on examining the bodies of those who have died of these affections, a certain number of tubercles, sometimes pretty large, in the lungs or bronchial glands, and more particularly in the latter. It is, however, probable, that eruptions of tubercles of this kind are almost always of small extent and rarely succeeded by others, and that they terminate favourably by the absorption or evacuation of the tuberculous matter ; since it is incomparably more rare to find phthisis supervening to fever, than to find tubercles in the lungs of those who die of this disease."

In conclusion, we have no hesitation in saying that the present translation exhibits a most complete view of the pathology and treatment of diseases of the lungs and heart, and is in all respects highly worthy of the valuable original.

Art. VIII.—*Reports of the Proceedings in the High Court of Justiciary.* By David Syme, Esq. Advocate. Vol. I. Part I. *from November* 1826 *to May* 1827 ; and Part II. *from June to December* 1827. Edinburgh, 1827. 8vo. Pp. 280. With an Appendix.

As this is unquestionably a work of great interest to the medical man as well as the lawyer, although more especially addressed by the author to the latter, we feel assured that a short notice of it will not be thought inconsistent with the objects of a Medical Journal.

Mr Syme's Reports include a notice of every case that is tried in the Justiciary Court of this city, with a more detailed account of all discussions on points of law, an abstract of the general evidence whenever it is important, and a full analysis of the medical evidence in every instance in which professional witnesses are examined. This is the first attempt that has been made by a lawyer to rescue from oblivion the medico-legal inquiries annually made throughout Scotland on the occasion of criminal trials. The value of the records which have thus been preserved in the course of a single year, will lead every medical jurist to lament that this ample field of observation and experience has been so long shut to science, and to hope that the author will be able to proceed with the same spirit and success with which he has begun. A collection of such cases as are contained in the numbers already published, if continued for a few years, will do more to raise medical jurisprudence from the station which it has hitherto occupied, to make medical evidence be respected by courts of law, and medico-legal studies be prosecuted by the medical profession, than all the declamatory complaints of unjust neglect which have for some years past been anxiously poured forth by its votaries.

To the medical jurist, and more particularly to those who have deplored the slow progress which the science of legal medicine has till lately made in this country, the evidence on the trials reported by Mr Syme must give unfeigned pleasure, as it bears the most ample testimony to the fact, which must likewise have been forced upon their attention from other sources,—namely, that in this part of Britain medical jurisprudence is now at length claiming for itself on all hands the regard to which it is so justly entitled. It will be seen from Mr Syme's Reports that our law officers are now sensible of the necessity of entrusting medico-legal investigations to competent hands; that even in remote country districts these investigations are now often conducted in an unexceptionable manner; and that, as a necessary consequence, the chief medical questions in the cases are answered with precision, and with much less of that odious discrepancy of opinion which has so long prevailed, to the disgust of courts of law and the disgrace of medicine. It is an event probably unprecedented in the history of the Justiciary Court, that in no less than three cases of poisoning, which have been made the subject of trial in succession, and which have been all of a complicated nature and have required much nicety of investigation, the question of the occurrence of poisoning, and all collateral points, have been decided without any material difference of opinion, and without the ingenuity of counsel having been able to throw any doubt over the accuracy and trust-worthiness of the witnesses.

The trials of which Mr Syme has given the medical evidence in detail are the following,—four cases of murder by wounds, three cases of murder by poison, one case of child-murder, a case of assault with intent to murder by strangling, and a case of the administration of poison with intent to murder.

1. The first case, No. 33, is that of *William Alexander* and *Janet Blackwood*, who were accused of having murdered the wife of the former by repeated blows with the fist and with va-

rious blunt instruments. It came out in evidence that the deceased was frequently struck on the face and body by both prisoners, and in particular that, while she was sitting on a chair in a state of beastly intoxication, her husband struck her a severe blow with the fist near the left ear, in consequence of which she fell to the ground, striking the right side of her head against the cross bar of a table;—that, from that time or soon after, she remained in a state of insensibility till two days afterwards, when she died;—and that besides various contusions on the head, body, and limbs, and a fractured rib, there was found, opposite an ecchymosed spot on the right and upper part of the forehead, an effusion of coagulated blood under the dura mater, to the amount of four ounces.

Mr Newbigging, the principal medical witness, very properly ascribed death to this effusion, and the effusion to the injury received in consequence of the head having come in contact with the cross bar of the table. He admitted, however, that the injury might have been inflicted by a fall from the bed, if the head struck a hard body; and likewise that the effusion might have been caused by excessive drinking without any external injury at all. * But he concluded by saying he felt himself fully warranted from the circumstances, in ascribing death to the injury. Another witness was also examined, whose name we shall have the charity to suppress, but whose evidence was much too singular to be passed without notice. He stated that he could not think the chief injury was caused by any of the blows described by the other witnesses, the injury was so severe;—that such an injury would require a violent blow with some blunt weapon, or a fall from the height of a story or two;—and that he could not think it was caused by a fall, *because the room was so small;* besides that it was admitted *a drunk man fell lighter than a sober man.* To the serious part of this evidence it is enough to oppose a statement by Mr Newbigging, that " a blow on the head, though not very severe, often produced serious mischief within." The prisoner Blackwood was acquitted, and Alexander upon Mr Newbigging's evidence, that the fatal injury was caused by the fall, not directly by the blow, was found guilty of culpable homicide, and sentenced to fourteen years' transportation. We confess we cannot exactly enter into the nice distinction which in a case of this kind the law draws between the direct injury caused by a blow, and the indirect injury caused by a fall, which is received in consequence of a blow.

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* We heard at the time some surgeons doubt the accuracy of the witness in this statement, on the ground that they had never personally witnessed extravasation in any dissections of persons killed by excessive drinking. Mr Newbigging nevertheless was amply borne out in his opinion by the records of medical jurisprudence: In Bernt's Contributions no fewer than four examples of extravasation are related, (*Beiträge zur gerichtlichen Arzneikunde*, ii. 59. iv. 38.) So rash is it to rely, as many would have witnesses do, on pure personal experience.

2. The next trial we shall notice, No. 38, is that of *Allan Grant, James Stewart, Mary Muirhead,* and *Isabella Kerr,* for the murder of Mr Dow, and for robbery and theft, a mysterious case, which made a great deal of noise at the time. It need not detain us long, however, as the medical evidence threw little light on the circumstances of the alleged crimes. The theft was fully proved; but there was a complete want of moral evidence in support of the charge of murder. What little was procured, indeed, favoured the presumption of accidental death; for the deceased was much intoxicated at the time of his death; his dead body was found at the bottom of a dark stair, and the cause of death was discovered by medical inspection to have been fracture of the fourth cervical vertebra. Mr Newbigging, the chief medical witness on this trial also, deposed, that the appearances of contusion found by him on the head were such as might have been produced by a fall; that the fracture was a very likely accident to be produced by such a fall; and that it could not be produced in any other way, except by a large heavy body falling on the head, or " some very heavy instrument used with some extraordinary degree of force." The Lord Advocate, considering the evidence, especially of the medical witness, departed from the charge of murder. The prisoners were found guilty of theft and transported for various periods, according to their share in the theft.

3. The third trial to be noticed, No. 44, is a rare one in respect to several of its features. The most important question which was involved in it related to the responsibility of the prisoner for the death of the deceased, death having been caused by tetanus, the consequence of unimportant wounds. *Alexander Mackenzie* was indicted for robbing, assaulting and murdering Alexander Clerk; and the murder was charged to have been committed by numerous blows and wounds inflicted on various parts of the body, in consequence of which he languished for ten days and died. It was proved, that, although the deceased was stunned and confused by the maltreating he experienced, he nevertheless had not received any severe injury. On the third day after, however, he was attacked with incipient symptoms of locked-jaw, which increased so slowly that on the fifth, sixth, and seventh days he was able to go out, and on the seventh day particularly was out most of the day at his work. From that time the symptoms increased more rapidly, and he died on the tenth of true tetanus. On dissection the only appearance of note observed was inflammation of the throat and of the trachea and bronchi.

The medical witnesses examined were Mr Newbigging, Mr Liston, and Mr Mackenzie, besides a medical student and another medical gentleman who deposed to certain particulars of the deceased's illness. In reply to a question which was a good deal pressed on the witnesses, they all agreed that the tetanus was not caused by exposure to cold when the man was out on the fifth, sixth, and seventh days, but that it existed previously. It was likewise agreed on all hands, that the wounds must have been the cause of it, even though they were trivial, because it had been known in other cases to arise from wounds even more trifling. In relation to the *degree* in which the wound and tetanus stood to one another as cause and effect, it was farther agreed, that tetanus lay in the natural progress of events, just like mortification, although neither so necessary nor so common a result: In so much, in fact, was it not a necessary or common result of such wounds as the deceased received, that it never would have been looked forward to in the prognosis by any surgeon.

These opinions seem all perfectly correct. Perhaps the *accidental* nature of supervening tetanus should have been placed in a stronger point of view, namely, by the fact, that in forming a prognosis not only in a case like this, but likewise in any case whatever of external injury, it is next to impossible to regard tetanus otherwise than as a very rare and unlikely consequence.

The prisoner was found guilty of culpable homicide, and sentenced to fourteen years transportation, the Court holding, that *the wounds not being of an aggravated description, and there not being any proof of previous deadly malice or intention to kill*, the case could not be accounted one of murder. It is of importance that the medical witness and jurist should be distinctly aware of the effect of his testimony on the law as well as the facts of a case. We may remark in explanation, therefore, that in this instance the previous intent, which is indispensable to constitute murder, was not proved to have existed either by the evidence of the prisoner's words and actions before, during, or after the assault, or by the evidence derived from the wounds being in themselves severe or dangerous. Had this intent been proved either in the one way or the other, the case would have been one of murder; for it appears to have been the opinion of the court that tetanus, though an effect neither necessary, nor common, nor such as in like circumstances may be anticipated, was a consequence not sufficiently *accidental* to have relieved the prisoner from responsibility.

4. The fourth case, that of *Campbell* and *Helm* for culpable homicide, No. 88, possesses no particular interest. The deceased was killed by sustaining a fracture of the lower part of the occi-

put, attended with effusion, and the fracture was received in consequence of a fall from a knock-down blow among stones.

5, 6, 7. The three next trials, No. 34, and App. 1 and 2, are by far the most interesting in the work. They are the trial of *Mrs Smith* for the murder by poison of Margaret Warden,—of *Margaret Wishart* for poisoning her sister,—and of *John Lovie*, a farmer, for poisoning his servant Margaret Mackisser. Of the two first nothing farther need be said here, as they are already reported in detail in this Journal by Dr Christison. The third was exceedingly similar to the rest, being a charge of murder committed on a female who was believed to be with child to the prisoner. The case was tried at Aberdeen last autumn circuit. The chief medical witnesses for the crown were Messrs Jameson and Coutts, surgeons at Fraserburgh, Dr Blaikie, Professor of Surgery, Dr Knight, Professor of Natural Philosophy, and Dr Forbes, Professor of Chemistry at Aberdeen, and Dr Christison of Edinburgh. The proof of death by poison was complete. After some ineffectual trials made at the time of the inspection, the contents of the stomach and the stomach itself were divided into two portions, of which one was entrusted to the Aberdeen professors, and the other sent to Dr Christison ; and both succeeded in detecting arsenic unequivocally. In the face of very strong moral evidence the prisoner escaped with a verdict of *not proven*, probably because the jury did not consider the crime satisfactorily attached to the prisoner, while there was not some direct evidence of the administration of the poison. There is only one circumstance in the case which calls for special notice here. On the trial of Mrs Smith it was stated by Dr Christison that he found *bright yellow* particles on the surface and imbedded in ulcers of the stomach ; and he imputed these to the conversion of part of the oxide of arsenic into the sulphuret by sulphuretted hydrogen disengaged in the stomach. Dr Fyfe stated that he did not conceive this to be a probable explanation ; and Mr Jeffrey, the prisoner's counsel, elicited from Dr Christison on his cross-examination, that sulphuretted hydrogen had never been sought for in the human stomach after death. * The case of Margaret Mackisser sets this question at rest ; for Dr Christison again found shining yellow particles on the surface, and, instructed by experience, did not leave the existence of sulphuretted hydrogen to be presumed from analogy, but actually detected it in the bottle, in which the stomach was sent, by immersing in the bottle a slip of paper dipped in nitrate of lead. This appears in fact to be by no means an uncommon appearance in the stomach. It was remarked in another of Dr Christison's cases, (Edin. Med. Chir. Trans. ii. 284.) We have ascertained by experiment that the solid oxide is converted into sulphuret by sulphuretted hydrogen if moisture is present. This might have been presumed; but being doubted, it is right to determine it expressly.

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* See Report of Mrs Smith's Trial, *passim*. Vol. xxvii.

8. The trial for child-murder, No. 95, was followed by conviction, a rare result now-a-days; but the circumstances were different from those of the ordinary instances of child-murder, the child being seventeen months old, and the accused person being the father.—*James Glen* was indicted for the murder of his natural child by drowning it in the Forth and Clyde Canal, or by first strangling and then drowning it. In such cases the medical evidence is often unsatisfactory, because the signs of strangulation as well as of drowning are not unfrequently obscure, particularly in children. The present case was of that nature, for it is not easy to see how the medical witness was borne out in the strong opinion delivered in the report and on the trial. The child was carried away from its mother by the prisoner on the 1st May, was found dead in the canal on the 9th, and was inspected on the 10th. The identification of the body was established in part, and very satisfactorily by the medical evidence; for it was proved, on the one hand, that though the body was in some places considerably decayed, the face retained its natural appearance; and on the other hand, that certain marks produced by burns, which were referred to by the mother and others, existed precisely where they were described to be. The inspectors further found, " on the fore part of the neck, over the windpipe, a softish furrow, with a hardened ridge both above and below; an appearance such as would be produced by violence applied to effect strangulation;" and after explaining that in some cases of death by strangling or drowning no characteristic appearances are detected in the body, they conclude that, although they cannot speak positively, they think it probable that forcible means were employed to strangle the child. On his examination the chief medical witness " leant strongly to the opinion that strangulation was the cause of the child's death." Other particulars observed during the inspection, and not specified in the report, may have supported this opinion. But the report and oral evidence of the *medical* witnesses certainly does not state any circumstance whatsoever by which death by strangling could be distinguished from strangling effected intentionally or accidentally *after death.* Altogether the report does not appear to us sufficiently detailed or explicit. In all complicated and equivocal cases, such as this must be admitted to have been, it is of the utmost consequence that the inspector specify all the particulars of his examination, since it is well

known to medical jurists that the issue of investigations of the like kind often turns upon facts which at first appear to the inspector of insignificant importance. The prisoner, we have remarked, was condemned, and the sentence was put in execution. The moral evidence was exceedingly strong, so that even a presumption of violent death from the medical circumstances was sufficient for condemnation.

9. The next case,—a trial for assault with intent to murder by hanging or strangling,—deserves to be noticed, not so much on account of the medical evidence, which was exceedingly short and simple, as for the singular circumstances under which the crime was alleged to have been committed. The prisoner, *Marion Brown*, an old woman of 69, was accused of having attempted to hang her husband, who was some years older. This variety of homicide is stated in works of medical jurisprudence to be one which can be successfully attempted only by a person very superior in strength to his victim, and is in fact very rarely resorted to except by several persons conjunctly. In the present instance, the prisoner contrived to twist a small rope three times round the neck of her husband, while he was asleep, and to tie him up to a beam in the room, in such a manner, that, when the neighbours entered, the man was found lying on the floor with the head raised about a foot from it. He was quite insensible, the hands were dropped down powerless by his side, his face was livid, and it was some time before he could be roused; so that little doubt existed that he would have perished had he remained as he was two or three minutes longer. He deposed that he went to bed quite sober, having drunk no more than his share, with five other people, of three gills of whisky; and that *he was not aware of any thing which passed during the attempt to hang him*, or after it, till the surgeon succeeded in restoring him to his senses. The possibility of strangling a person during sleep, without his ever becoming conscious of it, and consequently without his being able to struggle, has been questioned by medical jurists. The case now quoted will show that such a thing is quite possible; and that, contrary to what is generally believed, a person may strangle one much stronger than himself, without any assistance.

The prisoner was proved not to have entertained decided previous malice against her husband, and to have been at the time of the act in a state of intoxication. The prosecutor, therefore, departed from that part of the charge which accused her of intent to murder, and she was sentenced to eighteen months of Bridewell.

10. The case of *Mary Anne Alcorn*, the last we shall mention, is novel, as being one of the few, if indeed not the only

instance in modern times, of the crime of poisoning having been substantiated without the discovery of the poison, nay, without any distinct evidence what particular poison had been used. The prisoner, a servant girl, sixteen years of age, was accused of having mixed tartar emetic or some other poison with the meat and gravy of which her master and mistress partook at dinner,—in consequence of which they were affected with violent symptoms of irritant poisoning, even to the endangering of their lives. A considerable part of the meat was left, and was examined chemically by Dr Turner and Dr Christison, without any poison being detected. But it is necessary to add, what has not been mentioned in Mr Syme's report, that, from the particular way in which the suspected substance was mixed with the meat, it must have happened that all, or nearly all of the poison was taken by the family at dinner. The whole particulars of the case were very important in a medico-legal point of view; but they are cursorily mentioned in Mr Syme's work, and even upon the trial they were not gone into with that minuteness which is necessary for enabling the reader to see clearly the grounds of the opinion delivered by the medical witnesses. Our limits will not permit us to supply the deficiency. We can only mention that one of the witnesses, Mr Thomson of Portobello, who attended the family in their illness, deposed,—that he had no doubt they suffered from the effects of poison,—that he could not say what the poison was,—but that he suspected it was some preparation of lead. The other witness, Dr Christison, deposed,—that, from the pure medical circumstances, poisoning was highly probable, if not certain,—that his reasons for this opinion were the nature of the symptoms, the time at which they began after a suspected meal, the fact of *three* persons having been similarly and simultaneously affected (for the prisoner also took a little of the meat and was slightly ill), and lastly, the fact, that all those who were taken ill suffered in proportion to the quantity they had taken of the particular dish which was suspected. He was inclined to suspect tartar emetic to have been the poison administered; but spoke doubtingly on that point.

The prosecutor, in consideration of the moral evidence, gave up the charge of intent to murder, and the prisoner, having been convicted of administering a deleterious substance with intent to do grievous bodily harm, was sentenced to twelve months confinement and hard labour in Bridewell.

The account we have given of Mr Syme's labours will convey some idea of the addition which is likely to be made to our medico-legal information, by the continuance of them. We have just one word to say with regard to the accuracy of the

Reports. Mr Syme gives only an analysis of the medical evidence, although a pretty full one. That this has been done with ability we can testify from ample means of knowledge. Still it is an arduous undertaking for an unprofessional man to give an abstract of medical opinions and statements. The alteration, omission, or interpolation of a single word, may change entirely the meaning of a passage, although the extent of the change is not apparent to the reporter. Some errors were therefore unavoidable; and, accordingly, we could point out several passages where, if our recollection serves us correctly, errors of this kind have crept into Mr Syme's Reports. They are inconsiderable, it is true, and only upon collateral points, not very directly influencing the leading opinions delivered; but still they are such as would lead a medical jurist to conceive the witness spoke loosely or in ignorance. On the whole, however, as we have already said, the abstract of the medical evidence is remarkably accurate; and greater accuracy could not well be insured, unless the evidence was reported in the very words of the witnesses.

We must not forget to mention, that the author has in every case published the written medical reports verbatim, and several of them are very important. A natural consequence of this practice will be, that medical inspectors, for fear of having their carelessness exposed, will pay more attention to the framing of the reports, which are often miserably defective. If drawn up as they ought to be, they should constitute by far the most valuable part of the work to the student of medical jurisprudence.

ART. IX.—*Researches into the Causes, Nature, and Treatment of the more Prevalent Diseases of India, and of warm Climates generally. Illustrated with Cases, Post Mortem Examinations, and numerous Coloured Engravings of Morbid Structures.* By JAMES ANNESLEY, Esq. of the Madras Medical Establishment, late Surgeon to the Madras General Hospital, M. R. C. S. and M. R. M. S. In Two Volumes. Vol. I. Large Quarto. Twenty-one Coloured Engravings. London, 1828.

IT has been generally understood among those of the profession who are interested in the progressive improvement of medical knowledge, that our colonial possessions in the East afford most abundant means of instruction on many points on which little accurate information can be obtained in Europe. Many diseases little prevalent or little known in temperate countries, the tropical practitioner has frequent occasion to treat in all their varieties. The maladies of the chylopoietic and assistant chylopoietic organs, especially, are so frequent, so rapid in progress, and so formidable in their effects, that their occurrence gives a new character entirely to medical observation and practice. Thus the true nature of dysentery, cholera, and the various diseases of the liver, which, if known in this country, are comparatively mild in character and rare in occurrence, can be studied no where to advantage save in tropical climates, in which they occur on the great scale.

Notwithstanding these advantages, however, our oriental possessions have not till of late been very productive in medical knowledge. Since the time when the work of Lind appeared, the only writers who attempted to communicate the result of their observations were Girdlestone,* Hunter,† and Curtis.‡ It is further remarkable, that till recently, however zealous the servants of the company may have been in collecting, they have shown no particular eagerness in communicating information. For the only information on Indian diseases which Europe has received, the professional reader has been indebted to the exertions of such military surgeons as Marshall, Ballingall and Bampfield.

Now, however, it must be matter of delight to every one who has at heart the progress of the healing art to observe, that a new era has commenced among the medical officers of the East India Company. Not only are several of these gentlemen distinguishing themselves individually by their efforts to obtain more correct knowledge of the diseases most common in the varied and extensive regions in which their professional services are required; but the foundation of the Calcutta Society, by giving concentration and direction to the efforts of many, has already afforded encouraging proof both of the zeal and the capacity of our brethren in the East for improving the art. The official reports presented to the Presidencies on the epidemic cholera, have been formerly noticed as examples of a similar spirit of diligent research.

Of all those who are labouring thus to distinguish themselves in the acquisition and dissemination of useful knowledge, to no one, so far as we have hitherto seen, will a larger portion of gratitude be due, not only from this country, but especially from his medical brethren and successors in India, than to Mr Annesley. The inquiries of this gentleman in relation to the diseases of India, and especially those on cholera, we had occasion to bring before the notice of our readers in terms of high approbation. At present he appears again before them under circumstances which entitle him to still greater attention, and, if possible, to a larger share of regard.

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* Essay on Hepatitis and the Spasmodic Affections of India. By T. Girdlestone. London, 1787.

† An Essay on the Diseases incident to Indian Seamen, or Lascars on long voyages. By William Hunter, A. M., &c. fol. Calcutta, 1804.

‡ An Account of the Diseases of India, as they appeared in the English Fleet, and in the Naval Hospital at Madras in 1782 and 1783, &c. &c. By Charles Curtis, former Surgeon of the Medea frigate. Edinburgh, 1807.

The object of the present work is to investigate the causes and nature, and to determine the treatment of the diseases prevalent in India particularly, and in warm climates in general. This task Mr Annesley must be admitted to have accomplished, so far as he has hitherto gone, in a manner neither cursory nor superficial. Accustomed for the space of twenty-five years to the habitual observation of the most frequent Indian diseases, he has spared no pains in ascertaining, in the most authentic manner, either the circumstances under which they originate, the effects which they produce upon the organized textures of the body, or the best method of counteracting their effects and bringing them to a favourable issue.

The present volume, which is the first of the two in which we understand the whole work is to be comprised, consists of three books or general divisions, twenty-one coloured engravings, and three appendixes on various topics relating to the materials from which the author derives certain parts of his information. Each book is divided into a number of chapters, sections, and subsections, according to the nature of the subject under consideration.

The first book may be regarded as a general introduction to the whole work, in so far as the author takes a view of all those circumstances, which long and accurate observation shows exercise a powerful influence on the production of disease in warm or intertropical countries. After some judicious observations on the labours of previous authors on tropical diseases, on the causes of the contradictory statements which have been given on these subjects, and on the means by which this discordance is most likely to be rectified, the author proceeds in the first chapter to take a view of the digestive and assimilative functions in the healthy state, and of the part which each organ performs in the different processes of which digestion and assimilation consist.

In the second chapter, in which he takes a general survey of the causes productive of diseases in warm climates, he proceeds more directly to the investigation of his subject in a series of sections and subsections. These causes, in imitation of Lancisi, Lind, Rush, Foderé, Brocchi, Montfalcon, and others in modern times, he traces chiefly to the operation of solar heat on the soil, when more or less moistened with water, so as to produce the exhalations known under the general name of *miasma* or *malaria*.

The view of this subject which Mr Annesley takes, though in imitation of that of Lind, is, however, more complete, and derived from a more comprehensive and perhaps more accurate survey of the surface of the globe. The different sources of miasma or malaria, he refers to the following heads: 1*st*, marsh lands and the low grounds along the banks and at the mouths of rivers; 2*d*, the low, dense, luxuriant copse-wood called jungles in the East; 3*d*, forests situate in valleys; 4*th*, rice-grounds; 5*th*, canals and ditches, pools, and lagoons.

The situations most favourable for the formation of this product he shows to be the low grounds in the bottom of valleys and along the banks or near the mouths of rivers, whether they be alluvial islands, extensive and multiplied interamnal deltas, or mere marshy tracts; and this character he applies especially to the tracts contiguous to the Jumna, the Ganges, the Burrampooter, the Irrawaddy, and other rivers of India. Mr Annesley follows the greater part of authors in ascribing much of this malaria to the abundant materials derived from the incessant decomposition of animal substances with which warm countries particularly abound. He maintains strenuously the great influence of salt marshes, and mixtures of sea water with fresh, in situations in which they are liable to stagnate, in generating this unhealthy product. This effect he does not ascribe, as some have erroneously done, to the smaller proportion of salt which water in such circumstances must contain; and he justly remarks, that it does not follow, because a large quantity of salt is antiseptic, that a small quantity must promote putrefaction. He thinks he is justified in concluding it to be a fact, that sea-water mixed with fresh water and vegetable matter in a state of decay, under the influence of intense solar heat, increases the generation of effluvia, and renders them more concentrated. This result again he ascribes to the greater quantity of animal matter which sea water contains, and which disposes it to more rapid putrefaction when subjected to high temperature and kept at rest. The last circumstance is in all probability the one which operates most peculiarly and most powerfully. For it may be remarked, that in all situations, in which sea water is poured among fresh, or made to stagnate on the coast, its perfect quietude seems to be the most universal and invariable consequence. This is the case when sea water breaks in at spring-tides over level tracts of coast, and remains undisturbed generally till the next change or full moon, sometimes to one or two after. It is also the case when the mixture of sea and fresh water in rivers is forced, either by high winds or unusual tides, or other causes, over their banks, and left there undisturbed, occasionally till the next extraordinary tide or the next high wind.

Another abundant source of insalubrious exhalations are dense low *jungles*, as they are termed in the East, or copses of thick and luxuriantly growing wood and vegetables, generally more rank from the circumstance of growing in low humid situations. Every thing in these copses must contribute powerfully to the generation of unwholesome air. The thickness, exuberance, and succulency of the plants shooting between the dense brushwood and reeds, furnish a constant supply of decayed materials, as the unremitting vegetation proceeds, and prevent either sun or wind from reaching the soil. The roots, the creeping plants, either growing or decaying, and the exuviae of numerous insects and reptiles mixed with a rich absorbent soil, which they are incessantly increasing, and environed with a moist stagnant atmosphere, which is perpetually receiving fresh accessions of exhalation, without much means of renewal, which, indeed, the low confined situation renders difficult, if not impossible, are circumstances which must powerfully tend to favour the formation of malaria and morbific exhalations.

A third source of malaria admitted by Mr Annesley is that of extensive forests, especially when situate in, or extending into plains and valleys. In these are found many of the qualities possessed by the jungle. But they differ in this, that, while exhalations may be wafted to some distance from the latter, they rarely extend beyond the outskirts of the former. In admitting forests as a source of malaria, an anomaly is presented in the fact, that, clearing a country which ought to render it healthy, too often increases its insalubrity This anomaly Mr Annesley explains by considering the influence of intense solar heat on a rich vegetable mould, newly exposed and emitting, without interruption, its peculiar exhalations. Trees, he thinks, various circumstances show, are useful in intercepting or absorbing exhalations; and when removed, not only are the causes productive of these allowed to operate more forcibly, but they are exhaled more freely, and with greater chance of affecting the persons of human beings, residing or visiting in the neighbourhood. It must further be remarked, that forests are a source of malaria, principally as their outskirts, which resemble closely the jungle or copse, are more fully exposed to solar heat than other parts.

Rice grounds are so well known to be productive of disease, that it is unnecessary to dwell on them. In the south of Europe on the coasts of the Mediterranean, and in Carolina and Demarara in the New World, their operation is well known.

In the East the sickliness of the neighbourhood of Seringapatam from this cause is notorious. On these, however, and other sources of malaria, it is unnecessary to follow Mr Annesley farther.

In his observations in the second subsection, on the nature, properties, and effects of miasmata, he does not come to any very satisfactory conclusions. He does not admit the influence of carburetted hydrogen, which was found in the experiments of Gattoni, Moscati, Brocchi, and Julia, to be emitted from such soils. Assuming it to be a poison, he infers that it proceeds from "those elements which exist in a rich soil, and nourish the vegetable and animal kingdoms." These elements, he concludes, from the physical and medical history of terrestrial exhalations, when subjected to the action of the sun, with the joint influence of air and moisture, form new combinations, which are volatilized by the solar heat, and combine, mechanically we imagine, with the moisture present in the lower strata of the atmosphere. Many physicians have attempted to explain the physical circumstances of the atmosphere under which morbific exhalations are most likely to operate. That contained in the following passage is, so far as we are aware, the clearest that has been given.

> "While the vapours exhaled by the heat of the sun, and carrying with them the miasms given out by the soil, are kept in a state of rarefaction by the sun's influence, and conveyed to the higher regions of the atmosphere, then the usual effects of these miasms are not produced. But as soon as the heat diminishes so far as to permit the vapours to descend and become condensed towards the earth's surface, then the effects resulting from terrestrial effluvia become manifest. In the former case they are rarefied along with the aqueous vapour, so that they are incapable of making a hurtful impression on the system when received into the lungs; in the latter case they are condensed with the vapour, and precipitated to the lowest stratum of the atmosphere, and are exhaled into the body on a larger quantity, and sufficient in many instances to produce effects according to the circumstances of the individual. Hence it is that the evening and night dews are much more to be feared in all districts and countries subject to terrestrial effluvia than at any other period. This precipitation of the aqueous vapour and malaria forming mists and dews, particularly in low situations, is especially to be dreaded during still or stagnant states of the atmosphere. Within the tropics the earth retains its heat, after the sun has gone down, longer than the air. This circumstance leads to the accumulation of effluvia upon the surface; for, as their extrication from the soil proceeds to a considerable extent after the sun has gone down, they are no longer rarefied into the higher regions of the atmosphere, but are mixed with the dews and vapours descending at the same time, so that the sources, wherever exhalations are formed, being generally the lowest, are also the reservoirs where they accumulate. In warm climates their accumulation is

in some degree prevented during the land winds which blow through the night. But these winds are frequently too light to sweep away the exhalations which are collected in ravines and sheltered valleys; and when they are sufficiently strong for the purpose, they only serve to convey them to other places. It is chiefly during the calms which precede and follow the night winds that the accumulation of effluvia in low places takes place; and as these actions with their attendant vapours are usually about sunset and sunrise, exposure at these times should be avoided."

The application of these views to the explanation of the other phenomena of pestiferous exhalations, especially the respective salubrity and sickliness of places situate at different heights from the soil, or from the level of the sea, and at different distances from the source of exhalation, is next considered by Mr Annesley. As their application is pretty obvious, it is needless to repeat it here in detail.

In the third subsection on the effects of malaria on the human constitution, and on the mode of its operation, we find a correct statement of the ordinary opinions on this point. The view which Mr Annesley takes is, however, rather synthetical than analytical. Assuming it as an established fact, that malaria generates ague, remittent fever, and malignant continued fevers, he evades entirely the question, whether any of these forms of disease may originate from other causes? Even yellow fever he regards as the result of these causes operating in a very concentrated form on highly disposed subjects; and though he does not assert that oriental plague proceeds exclusively from this source, he goes so far, nevertheless, as to express the opinion, that miasmatic influence has a most powerful operation in the production of this disease. Fever, however, is not the only offspring of this agent; and dysentery Mr Annesley regards as very often the result of marsh exhalations. In proof of this he appeals not only to the testimony of Sir John Pringle, and of M. Vignes in relation to the dysenteric attacks in the French armies during their occupation of Italy and Germany, but to the authority of Bampfield, to the scorbutic dysenteries of Batavia, Rangoon, and among the forces sent on the expedition to Ava, and to that of the Milbank Penitentiary, which he conceives there are ample grounds for tracing to the same source. On the exact mode in which malaria produces this disease he gives no definite statements.

The influence of miasmatic vapours in producing disorder of the large glandular viscera, more particularly of the liver and spleen, is next noticed. Though it has been long known that these organs become enlarged, indurated, obstructed, and otherwise diseased in consequence of the fevers of marshy districts, Mr Annesley contends that they are often affected primarily without fever; and this inference he rests upon the fact, that within the tropics there is rarely seen a case of disease in which, upon dissection, the liver and spleen are both sound. In this, perhaps, there may be some source of fallacy, in the circumstance, that few persons indeed who have lived within the tropics have not suffered one or more attacks of fever, either in the intermittent, remittent, or continued form. From the observations of Lining, however, in Carolina, of Jackson in various parts of North America, from those of Hantesiercke and others in various marshy districts in France, and above all, from those of Foderé on the inhabitants of Lower Bresse, and of Martigues,—it appears that residence in marshy districts or in marshy air has a manifest tendency to induce enlargement and induration of the liver and spleen, with emaciation, and those appearances said by the older physicians to indicate the cachectic condition dependent on visceral obstruction. *

From various facts, Mr Annesley thinks it a just conclusion, that the malaria makes its injurious impression on the animal frame chiefly through the medium of the internal surfaces of the air-passages and the lungs. Whether, however, the material cause is absorbed, or operates on the nervous system only, he does not attempt to determine. He thinks it probable that it operates in both modes. On its peculiar operation nothing very precise is adduced.

In the fourth subsection, the author deduces from the principles previously established practical measures for preventing the generation of marsh exhalation, and obviating the effects on the human body. These it is unnecessary to enumerate, as they must be obvious to most readers who are aware of the agent itself. Mr Annesley thinks the chlorurets of lime and of soda, the disinfecting agents of La Barraque, might be highly useful in many situations in India, where towns are exposed to the noxious emanations of foul drains, sewers, and ditches.

The second section of this chapter is highly interesting. The author proceeds to take a view of the climate and usual course of the seasons in the British dominions in the East; and considers the peculiarities which pertain to the three Presidencies of Bengal, Madras, and Bombay. It would be vain in these pages to attempt any thing like a general abstract of this survey, which deserves the particular attention of every one attached to the medical establishment of the Honourable Company. We need only say, that the attention with which it has been digested, partly from personal observation, partly from authentic official returns, gives it a value which it is the lot of very few general sketches of this description to possess.

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* Traité de Medicine Legale et Hygiene Publique, par F. E. Foderé. Paris, 1812.

The facts given in this survey afford an ample confirmation of the general accuracy of the doctrine which ascribes the predominant maladies to the physical peculiarities of the country. Thus it appears from the official returns made to the Medical Board of the Bengal Presidency for a series of five years, that in that province, which abounds throughout with materials for malaria, fever is most frequent in the rainy and hot seasons; that dysentery and diarrhœa are more prevalent during the rainy and cold seasons; and that hepatitis and cholera are oftener met with in the hot season. The banks of the Hoogley and the adjoining district are very low, and have been long known to be particularly insalubrious; and in this district, and the contiguous one of Jessore, which are almost submersed during the rainy season, fever and dysentery prevail to an extreme. The sickliness of Diamond Harbour on the Hoogley is well known. Moorshedabad, the native capital of Bengal, a city of 200,000 inhabitants, is very unhealthy even to the natives; and for this its situation on a low level soil on both sides of the Cossimbazar, the most sacred branch of the Ganges, surrounded by jungle and swampy plains, sufficiently accounts. In Gundwana, as in other parts where thick jungle and swamp abound, bilious remittent fever and dysentery are frequent among Europeans; and agues, ulcers of the legs, diarrhœa, and rheumatism among the Sepoys and natives. Cuttach in the province of Orissa, which is much covered with wood and dense jungles, and intersected with streams, and during spring-tides almost inundated with sea water, is a remarkable illustration of the general principles stated in the first section.

In the presidency of Madras the hot and windy season of May, June, and July, are represented as generally most healthy; and though in certain seasons, from particular circumstances, sickliness may prevail most in December and January, or even in June and July, yet in general this is observed to be most prevalent about the commencement of the monsoon, or from August to November. According to the tables calculated from the returns to the Medical Board, the prevailing diseases are fever, dysentery, and hepatitis.

The greater prevalence of hepatitis and dysentery among the European troops in the Madras than in the Bengal presidency is ascribed to the greater warmth of the climate, and to the excessive use of intoxicating liquors. In Travancore, at the south-western extremity of the peninsula, diseases of the liver appear to be particularly endemic among the Europeans; and upon disorder of this organ dysentery is often found to depend. In the Mysore, the island of Seringapatam, formed by the separation and reunion of the Cavery, though upwards of 2000 feet above the level of the sea, is nevertheless so circumstanced, that it possesses many of the conditions necessary to an insalubrious climate. Lying in a deep valley, and consisting of rich, black, alluvial soil, through which during the monsoon water percolates freely from the river, as the whole of the small island is appropriated to wet cultivation, it forms for miles an uninterrupted swamp, from which previous to and during the south-west monsoon, the noxious exhalations are wafted in abundance to the inhabited parts. It is on this account apparently that the season which terminates with the north-east, and commences with the north-west monsoon, is mentioned in an able official report as the most fatal at Seringapatam.

On the comparative salubrity of the presidency of Bombay, Mr Annesley was unable to procure medical documents or returns. The insalubrity, however, of the place, and especially of one of its stations, is indisputably demonstrated by the following fact, which was communicated by Sir John Malcolm. His Majesty's 17th regiment of light dragoons served in India for a space of fourteen years, from 1809 to 1822, acting chiefly in the province of Gujerat, and while not on actual service stationed at Kairah. During the period of this service they lost 27 officers and 772 men, including trumpeters, non-commissioned officers, and rank and file. The annual average effective strength of the regiment was 730 men; and the loss in the period of fourteen years, therefore, was 42 men above their complement. The greatest mortality appears to have taken place in 1813, when they lost five officers and 166 men ; and it is rather a curious circumstance, that during the years 1816, 1817, and 1818, when they were employed on active field-duty, they lost fewer men than at any other period of their service. Nearly the same result regarding the insalubrity of this place is obtained from the returns from the 4th dragoons, who succeeded the 17th at Kairah.

In the survey of the physical surface and salubrity of Chittagong and Arracan, Mr Annesley adopts nearly the accurate account of Dr Macdougall, given in the Transactions of the Medical and Physical Society of Calcutta. Much the same character applies to Rangoon, on one of the mouths of the Irrawaddy, on a low swampy delta, which is at one period of the year either entirely inundated, or an extensive marshy beach.

In his subsequent account of the climate, season, and salubrity of Java and Batavia in the East, and of that of the colonies on the coast of Africa, the West Indies, and the Mediterranean, though ably done, it is unnecessary to follow the author. The results which he has given from the official returns to the Medical Board in this country are particularly valuable.

The fourth section on the diet and regimen of Europeans in India and other warm climates, contains some remarks which nothing but their obviousness could make to be neglected. They are highly judicious, but, we fear, will not be seriously listened to by those who are accustomed to associate the ideas of good living and enjoyment. Against the practice of consuming at breakfast fish, meat, and eggs, and at tiffen rich soups, roast meat, fowl, &c. with the usual allowance of wine, and occasionally malt liquor, Mr Annesley raises his voice decidedly and forcibly. The strong wines and all malt liquors he unequivocally condemns, and recommends a little claret or Rhenish, or pure water, if possible. He also, with great justice, represents the dinner of the Indo-European as excessive in quantity, and consisting of articles too stimulating and too highly seasoned for the late hour at which it is taken. Such a meal cannot fail to be followed by disturbed and unrefreshing slumber, and to lay the foundation for numerous disorders of the chylopoietic organs and of the system at large. We must, however, be satisfied with referring the reader in general to the section, in which he will not be disappointed in finding much useful information.

In the third and last chapter of this book, the author examines the train of symptoms which in general precede intertropical diseases, and impresses the necessity of early attention, with the view of preventing those consequences which will undoubtedly follow if neglected. These premonitory signs are sometimes so slight as to escape the notice, not only of the patient, but even of his physician. In general, they consist in those slight derangements of the digestive organs which are known under the general name of bilious; and if these are obviated by suitable management at first, no definite morbid state may take place. If, however, they are neglected, as often happens, the functions, not only of the alimentary canal, but of the circulation in general, are so permanently and completely deranged, as to constitute one form or other of tropical disease, which runs its course with more or less severity, according to circumstances. This incipient or premonitory derangement of the system may terminate in congestion of the hepatic system, in hepatitis, acute, subacute, or chronic, in bilious diarrhœa, or dysentery, or in bilious remittent, or continued fevers, as the nature of the predisposition of the individual, or the exciting or co-operating causes may determine. In such circumstances, Mr Annesley impresses strongly the urgent necessity of unloading the sanguiferous system in general, and the vessels of the hepatic organs in particular, and of restoring, by suitable attention to the chylopoietic organs, the natural state of the secretions to which they are subservient.

In the second book, which treats of the diseases of the stomach prevalent in India and warm climates, the author traverses a great deal of interesting ground. Passing over entirely the first chapter, on the functional or primary disorders of the stomach, we proceed at once to the second, in which Mr Annesley treats of the subject of inflammation, and organic lesions of the organ.

Inflammation of the mucous membrane of the stomach (*gastritis erythematica*,) is, Mr Annesley thinks, a very common form of disease in warm climates. It takes place to a greater or less extent in the advanced stages of dyspepsia; or, in other words, the dyspeptic symptoms are truly the effect of gastric inflammation. When it takes place in bilious fever, which it often does, he ascribes it to the flow of irritating bile. If neglected in this state, or treated, as too commonly happens, with tonics, it almost invariably proceeds to ulceration of the mucous follicles, which is generally indicated by increase of the pain, heat, and vomiting, by emaciation, and by the matters discharged being a dark-coloured grumous fluid. In this state the patient either wastes gradually, or is cut off suddenly after laceration of the bottom of some one of the large ulcers, when the contents of the stomach escape into the peritoneal cavity, and induce fatal peritonitis.

As this form of gastric disease rarely comes under the inspection of the pathologist till it has proceeded to ulceration, Mr Annesley says little of its primary anatomical characters. In cases in which ulceration had not taken place, the coats of the stomach have been found thinner and softer than natural, while various traces of inflammation were manifest. In others, to these appearances have been added ulcers of every size, and numerous in proportion to their minuteness. In other instances again, the gastric coats have been thicker and softer than usual, the organ contracted, and with marks of inflammation and ulceration. In the latter state the appearances accord much with those observed by M. Louis. The ulcers are generally most numerous near the pylorus, and at the cardiac orifice of the stomach.

In more acute forms of the disease, the external vessels which supply its peritoneal coat are generally gorged with blood, the capillaries are injected, and the omentum is more vascular than natural. The villous membrane is flaccid and collected into folds from constriction of the muscular coat; sometimes ecchymosed from transudation; generally of a colour varying from bright red or crimson to deep purple or a dark livid hue.

These discoloured spots, which indicate inflammation, are in the form of broad patches or wide bands or zones. The surface may be covered with viscid mucus, with yellow puriform thick matter, with white thick, concrete albuminous matter, or, more rarely, with thin ichorous or sanious fluid. The mucous tissue is thick, soft, and easily detached, especially over the blood-shot patches. The whole gastric coats are thicker than natural; and their capillaries are much loaded, which gives the organ a dark, violet, or purple tinge. When the mucous surface is abraded, a dark grumous fluid is found in the cavity. Dark spots which have been regarded as marks of gangrene, M. Annesley thinks to be consequent on death, unless when the individual has been poisoned by some acrid and corrosive substance. When the peritoneal coat is effected, it produces the usual albuminous exudation.

In the third section on some of the organic diseases of the stomach, there is a remarkable similarity between the observations of Mr Annesley and those of M. Louis. Thus the former author remarks the same dilatation of the organ, and attenuation of its textures, the same softening of the mucous membrane, often with ulcers, the lacerability or actual laceration, and also the same thickening which are so fully described by M. Louis. Mr Annesley also remarks the frequency of thickening of the gastric tunics in patients cut off by fevers, dysentery, and hepatitis, sometimes with softening, sometimes with induration of tissue, and occasionally with incipient ulcerations. Not doubting that this coincidence is accidental, we regard it as a proof of the accuracy of two different observers, that mutually ignorant, so far as we are aware, of each other's researches, they have obtained nearly the same results.

There is one point only which appears to have occurred to Mr Annesley, and of which no trace is found in the work of M. Louis. This is that, with thickening and induration of the gastric tunics, a substance varying in consistence from white of egg to that of cheese is deposited in the gastric cellular tissue. This substance, which is albuminous in appearance and character, is usually accompanied with enlargement of the follicular glands in the vicinity, with long-continued inflammation, and indicates the transition to what is termed *scirrhus* of the stomach. It is most frequent near the pylorus and cardia, in many cases constricting the apertures, and converting them into thick, hard rings, the interior of which is covered with ulcerated patches of various sizes. Where this has taken place it is unnecessary to say what must follow.

The coloured patches of the mucous membrane of the splenic portion of the organ are perhaps partly the effect of transudation, partly of capillary congestion propagated from the splenic to the gastric vessels. These patches, which vary from a deep red to a violet, purple, brown, or slate colour, are often observed in the stomachs of those who have died of remittent fever, and generally in those who vomited during life dark-brown or black grumous fluids. The inference from these facts is obvious.

The third book, which is the largest, occupying fully the latter half of the volume, is devoted to the examination of diseases of the liver and of the biliary apparatus. These the author confirms general observation in representing as endemic in the eastern hemisphere. Some idea of the prevalence of these disorders may be obtained from the statement given by Mr Annesley, that, in the different divisions of the Bengal army, allowing for contingent sources of error in the documents, the per centage of admissions, for various forms of hepatic disorders, appears to vary from 2 to 16, averaging between 8 and 9 per cent. in the effective strength. In the divisions of the Madras army, again, the admissions for hepatic diseases vary from 6 to 35 per cent. in the effective strength, the lowest rate being in the provinces approaching those of the Bengal Presidency, and the highest in the southern parched districts. As hepatitis is really more frequent on the Coromandel coast and in the southern provinces of India, where the annual range of temperature is highest, it may be inferred, Mr Annesley thinks, that diseases of the biliary apparatus, and especially hepatic inflammation, depend in a great degree on the nature of the soil and climate, and especially on the habitual high temperature of the place.

We are obliged, however, to pass over the entire first chapter on the various functional derangements of the biliary system; and proceed at once to the second, on inflammation and some of the organic changes incident to the liver, as matter more novel and interesting not only to our oriental readers, but also to those at home.

Hepatic inflammation Mr Annesley represents as very generally the consequence of one or more of the derangements to which the function of the organ is liable. Thus it may succeed not only symptoms of congestion in the vessels of the hepatic system, but ague, remittent or continued fevers, and dysentery.

Inflammation, when affecting the surface, generally spreads to the substance of the organ; and when it commences in the substance, it occasionally extends to the surface. Most generally the right lobe is the seat of inflammation; next in frequency the right and left lobes at the same time; and the left alone is least frequently the seat of morbid vascular action. In India, it appears, from the researches of Mr Annesley, the substance of the liver may be inflamed to the greatest possible extent, even to the production of several abscesses in both lobes, or of one large abscess in the right lobe, without any distinct marks of inflammation of the peritoneal covering, except change of colour derived from the subjacent parts. Abscess even may burst into the abdominal cavity without inducing inflammation of the serous membrane where it points, and therefore without effecting adhesion.

The distinction of the disease into acute and chronic, according as it is seated in the membrane or in the hepatic substance, Mr Annesley shows to be inconsistent with the characters of the disease as observed in India. Although inflammation commencing in the surface of the organ almost always assumes the acute form in this climate, as in temperate countries, yet conversely, when seated in its substance, it does not always assume the chronic form. On the contrary, acute inflammation may commence and be seated in the interior substance, though perhaps it more frequently originates in that and spreads to the surface. Chronic inflammation, nevertheless, may be confined to the hepatic substance only.

It is chiefly in cases of fever or dysentery in which hepatic inflammation has supervened not long before the fatal event, that opportunities of observing the anatomical characters of this disease have been obtained. When inspection takes place, in such circumstances, the surface of the inflamed part of the organ is of a bright red or reddish brown colour, and more vascular than usual,—occasionally covered with gelatinous lymph or firmer albuminous matter, connecting the surface to the contiguous parts. The subjacent substance of the liver is vascular and loaded with blood. That of the organ in general is red or reddish brown, and softer and more friable than natural. When it is firmer, a chronic stage of disease is generally indicated. The surface is occasionally marked with red, brown, brick-coloured, greenish-brown, or black spots and streaks. A section by a sharp instrument presents a light-coloured mesh, studded with red or brick-red granules, and the divided ends of blood-vessels and bile-ducts.

Gangrene of the liver Mr Annesley has not seen in India; and he thinks the green, dark-red, or black shades above-mentioned, have been mistaken for this change, and given rise to the idea of termination in gangrene.

The common and hepatic-ducts, and occasionally the cystic duct, Mr Annesley has seen reduced to a small cord, and the canal obliterated, or at least constricted. In this country slight inflammation of the ducts inducing swelling and a degree of stricture is no uncommon cause of jaundice.

Abscess is a termination not uncommon of inflammation either acute or chronic. When this takes place, the liver presents various appearances in shape, according to the particular part in which the abscess is seated. When in the right lobe, which is most general, the lobe is more or less enlarged, according to the extent of the purulent collection. When either near the middle of the lobe, or approaching the upper surface, the lobe assumes more or less of a spheroidal or globular form, and rises high into the right side of the chest. Two or more abscesses give the organ a more irregular shape, according to the region which they occupy. The colour of the surface of the organ varies. It may be pale orange, pale or deep yellow at the point, especially, where the abscess approaches the surface; while the parts at the base are of a deep reddish-brown, or approaching to black. The apex of the tumour is occasionally mottled or marbled. The surface of the organ generally is deep-brown, or of dark colour, marked with spots. In chronic cases, the brown or deep-coloured surface of the organ is studded with minute collections of purulent matter, which seem to have been formed between the hepatic tissue and its peritoneal covering.

The matter contained in the abscess may be thin and watery, with thick curd-like clots floating in it; or homogeneous purulent fluid, varying, however, in consistence. Its colour varies from the usual yellow, or cream-colour, to yellowish-brown, greenish, green-brown, or green-yellow. It may be either contained in a distinct cyst more or less organized; or in broken down ragged hepatic substance without cyst; or in an indurated layer of hepatic substance. The contiguous substance, from the report of Mr Annesley, appears to be generally brown-red or brick-colour, vascular and softer than usual in the acute forms of hepatic inflammation. In many cases, however, Mr Annesley mentions, the hepatic substance presents few signs of increased vascularity or redness, until the immediate vicinity of the abscess is approached, when a deep-red zone, becoming successively deeper, is found to inclose the abscess. In such cases there is reason to believe the inflammatory action had been transferred by means of the suppuration to the spot forming and containing the abscess. This is corroborated by the circumstances attending what may be named the third variety of this form of hepatic abscess. " In other cases, evidently of a more chronic kind, little or no inflammation is observed in the vicinity of the abscess, excepting in the inner surface of the cyst, or in the hepatic substance in which the matter is contained." In other chronic cases the colour of the hepatic substance is said to be little changed from natural, being merely deeper, with some greater firmness. When such a portion of liver is divided, several small abscesses containing thick purulent fluid, varying from the consistence of cream to that of soft cheese, are found scattered through the substance of the organ.

From the successive changes remarked in these different forms of hepatic abscess, we think it not unreasonable to infer, that they are the result of inflammation varying in acuteness and violence nearly in the order now stated. The first may be viewed as the effect of the most acute form of inflammation; the second of one less so, or a subacute form; and the third and fourth of varieties of chronic inflammation.

These several varieties of hepatic abscess, and indeed every form of hepatic disease, are beautifully and accurately illustrated in the engravings, which are coloured most faithfully to the natural appearance of the objects. The reader is presented first with the distinct abscess, as it appears at the surface through the peritoneal coat; then with a longitudinal section of the same change, showing the transition from inflamed to suppurated liver; then with various forms of abscess, after acute inflammation, after chronic, and after organic induration. Next he is shown the minute purulent deposites on the surface and within the substance of the organ; the hepatic abscess extending into the lungs; the abscess of the right lobe *in situ;* afterwards diseased liver with inflamed intestines; atrophied liver (the brick red) with cicatrices; diseased liver with the same; tubercular disease of the organ; congestion with organic disease; congestion with enlargement; ulceration; scrofulous abscess; hydatids; and granular induration.

We regret that our limits will not allow us to enter into the history of the symptoms of hepatic inflammation in its several stages, as traced by Mr Annesley. Like every thing in this volume, it is evidently drawn up with great care from the most authentic sources,—cases in which the progress of the disease was observed from its origin to its termination in health or necroscopic inspection. Excellent, however, as it is, we must be satisfied with a general recommendation of it to the diligent perusal of the reader. To this we trust that the sketch, which we have thus hastily attempted, will furnish a strong inducement. Whether it does so or not, we can truly say, that no work will more highly reward the reader by the information which it contains. This is said chiefly to the European physician, who will study this work principally as a branch of general medical knowledge.

To the East Indian, and to all those whose professional pursuits or other relations lead them to tropical or even warm countries, it is needless to use any other language, than simply to say, that the information in the work of Mr Annesley must be indispensable to the conscientious exercise of their art. It may be indeed said, that hitherto disease has been treated, and physicians have done their duty without such a work. If this plea have any power, it must be small indeed, compared with the stronger and more imperative one,—that many of the disorders peculiar to the East have been misunderstood in nature, and trifled with in treatment; that many valuable lives must have been sacrificed from want of due knowledge of the characteristic signs of the incipient forms of destructive disease; and that many of the worst of these maladies are perfectly within the control of medical means, when employed with decision and promptitude, and in due season. If these considerations have no weight it is unnecessary to add more.

The only objection to the work is, that it is published in a form which necessarily places it within the reach of a small number only of the profession. As the nature of its contents renders it useful to all and sundry professional persons in the East, it may become a matter of expediency, if not of necessity, for the author to publish the letter-press at least in an octavo form. It is obvious that its present size, as well as its expence, must make it rather an inconvenient book for the library of a regimental surgeon.

ART. X.—*A Corn. Celsi Medecinæ libri octo ex recensione Leonardi Targæ. Quibus accedunt, &c. &c.* Concinnavit EDVARDUS MILLIGAN, M.D. S. A. S. S. &c. &c. &c. Edinburgi, 1826.

IT is rare, indeed, that we can venture to take notice of new editions of works which have been already long before the public. The great number of new works on various subjects, which necessarily claim more or less attention, renders this a matter of no easy performance.

Various circumstances, however, induce us to deviate from our usual custom in regard to the work, the title of which is prefixed to this notice. In the first place, we understand it to be generally admitted, that the work of the Roman physician abounds with so much excellent matter, shows so much accuracy of observation, and exhibits such a fine application of the general principles of sound reasoning and good common sense to medicine, all expressed in the most elegant and select language, that, antiquated as some pretend to rate it, it is a work with which physicians of reflection and taste have at all times been proud to avow their acquaintance.

The necessity of presenting the text of an author so distinguished in as pure a state as possible is obvious. This is not the place to consider the merits of the different editions of Celsus. It is sufficient to say, that to the one which was published in 1769, by Leonard Targa, a learned physician of Padua, after an elaborate collation of the most creditable manuscripts, is generally assigned the palm of superior accuracy. Whoever reads the Epistles of Morgagni on this subject is aware how corrupt the text was previous to the time of this able scholar, and must know what a benefit was conferred on the literary world by the classical labours of Targa.*

The edition of this editor, which has thus been regarded as the model of perfection, has been repeatedly reprinted. As occasionally happens, however, with such reprints, every successive edition was less correct than the one which preceded it; and, had the system been continued, it is truly difficult to say to what extent this species of corruption might have proceeded.

With the laudable intention of obviating these evils, Dr Milligan of this city, a gentleman distinguished not only for his professional attainments, but for much accurate classical knowledge, has presented to the lover of medical literature the present edition of Celsus, according to the most correct text of Targa. However humble this labour may be regarded by many who neither know nor value the attainments by which only it can be accomplished, it would be difficult to mention a more disinterested example of the application of medical knowledge and classical learning, or one for which the student ought to be more grateful. The text is throughout correct to an extraordinary degree. We speak from actual trial of a large proportion of the volume, when we say that we have not been able to detect a single error either in typography or punctuation.

Besides presenting the text in this highly accurate form, Dr Milligan has improved the present edition by many useful services. Thus he has given the synonyms of the diseases described by Celsus in the nomenclature of Sauvages and Cullen. He has attempted to ascertain the synonyms of various substances in the ancient *Materia Medica*, and given them in the nomenclature of Linnæus, Thomson, Werner, and the Edinburgh Pharmacopœia. And he has added throughout marginal notes on the subjects treated by the author,—a method of much use to the student in confining his attention to the subject. Dr Milligan has also given tables of the weights and measures of the ancients according to Pliny and Celsus; a synoptical view of the *materia medica;* and an explanation in figures of the ancient baths, and of certain instruments used in the treatment of diseases.

To conclude, this edition of Celsus is the best which has been published since the original one of Targa; and it may be justly recommended to the attention of the student.

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* Jo. Baptistæ Morgagni in Patavino Gymnasio Primarii Anatomes Professoris in Aur. Corn. Celsum et Q. Ser. Samonicum Epistolæ; in Quibus de Utriusque auctoris variis editionibus, libris quoque Manuscriptis et Commentatoribus, disseritur Hagæ Comitum, 1724.

[Addition to the Original Communications.]

Art. XX.—*Notice of the Appearances in the Brain of a Young Female who attained her seventeenth year without giving Indications of Sensation or Motion from Birth.* Communicated by Davies Gilbert, Esq. M. P., President of the Royal Society.

THE young person who forms the subject of the following notice was somewhat prematurely born, and appeared at birth to be very feeble. She gave no indications of the usual instinct which guides infants to their natural food; and much difficulty was experienced in teaching her to take the breast. One eye was observed to be smaller than the other, which appeared to be of the natural size.

Except this excessive weakness in the muscular motions, and total deficiency of the ordinary sensations and appetites, nothing particular was remarked for several weeks after birth, when convulsive fits took place, and increased in violence and frequency of recurrence till they exceeded one hundred in twenty-four hours. Every method that could be suggested by various medical attendants to counteract these fits of convulsions was tried without success.

She nevertheless continued to grow in length much like other children. But the defects in motion and sensation increased and became more apparent. Though she originally gave indications of sight, blindness came on; and cataract was observed in the eye originally of the natural size. The sense of hearing, if it existed at all, was very obtuse. Latterly it was impossible to doubt that she was quite deaf. Though she got a first set of teeth, these were not succeeded by others, so that they were temporary only. The sense of taste seemed to be tolerably perfect, as fruit, confections, custards, and other sweet articles were received with indications of pleasure.

She never acquired any thing like the power of voluntary motion; and she attained her seventeenth year without ever having held up the head, raised her hand to her mouth, or set her foot to the ground. She never was heard to utter any thing like an articulate sound; and a feeble cry or whine only was believed to indicate want of food. When this was gratified, especially with sweet articles, the countenance exhibited an expression of satisfaction.

The bowels were habitually very slow, so as to require the perpetual employment of medicine; and to this no temporary exception was ever remarked. She never acquired any command over the sphincters.

For some short time previous to death the weakness seemed to increase; and exactly on the day when she completed her seventeenth year, she expired in a manner so gradual, that the attendants thought her asleep, when she was really dead.

I understand that some indications of the menstrual discharge had been observed; and the usual marks of puberty were not wanting. I am not aware, however, that the mammæ had enlarged.

Her length must have been equal to that of many young persons of her age. The spine and limbs were distorted; and the person was much emaciated.

Inspection of the body after death unfolded the following appearances, which are given in the words of the surgeon who performed the dissection.

"On opening the cranium, the first thing which presented itself to observation was the very great distension of the vessels of the brain and of its membranes. The first longitudinal sinus was particularly gorged, and marked its course by a raised dark blue line extending across the dura mater from before backwards. We laid it open, and found it filled with coagulated blood.

"On proceeding with the dissection, we found some little water in the lateral ventricles; but this is often poured out after death in the healthy subject; and as the contents of the ventricles were perfect, more particularly the optic thalami, I should be inclined to consider that as a natural appearance.

"On removing the brain from the basis of the scull, so as to expose the nerves, we could trace each pair taking their usual course to their destination; and they all appeared firm and healthy, as in the most intelligent individual. But in the base of the scull itself, not a vestige of the dura mater was to be seen. Its place was supplied by a thin semitransparent membrane, very lax and irregular, so that it afforded no protection to the nerves in their exit from the scull.

"On the back part likewise the whole, or the greater part of the *tentorium* was deficient, thus allowing the whole weight of the brain to rest on the cerebellum.

"We proceeded carefully to trace the nerves to their separate organs, and found them in every respect seemingly perfect. Indeed the optic nerves appeared particularly developed, as also the ophthalmic branches, although cataract existed in both eyes to a very considerable degree. Nothing further appeared worthy of observation. From these circumstances, I should be inclined to say, (as far as human research can explain the mysteries of our nature) the causes of the child's deficiency were as follow.

"That from the want of the *dura mater* on the base of the scull, and its place being only imperfectly supplied by the thin expansion before mentioned, the mass of brain was allowed to press on the nerves of sense as they passed through their separate foramina, and thus their vital principle became destroyed;

and that the cerebellum, not having any protection, in consequence of the want of tentorium, was also pressed upon by the cerebrum, which likewise suffered in its turn, thus probably accounting for the total extinction of intellect."

(Signed) James Coles,
Member of the Royal College of Surgeons, London.
Feb. 1826.

The defect of tentorium and of the spheno-occipital portion of the *dura mater* in the case now related is important in directing the attention to the probable influence of one part of the organ pressing or gravitating, as is said, on another. That this influence, however, may not be overrated, it is requisite to mention that Sir Anthony Carlisle published many years ago in the first volume of the Transactions of the Society for the Improvement of Medical and Surgical Knowledge, the history of a woman apparently near 20, who died with symptoms of fever in the Westminster Hospital. In the brain of this woman the *falx* was entirely wanting from the *crista galli* to the *tentorium*, the opening in which also was much larger than usual; and consequently, not only did the one hemisphere press the other, but part of the brain gravitated on the cerebellum. Yet in this woman the intellectual powers were said to be perfect; and she had no particular complaint of her head. Vol. i. p. 212.

Art. XXI.—*Case of Pertussis, with the Dissection of the Body, in which was found, besides the Morbid Appearances, a Remarkable Transposition of the Thoracic and Abdominal Viscera.* By William Moncreiff, M. D. F. S. S. A. Fellow of the Royal College of Physicians, Edinburgh, &c. (Read to the Medico-Chirurgical Society of Edinburgh on the 5th March 1828.)

E. C., a girl, æt. 4½, became my patient on the 19th January last, affected with pertussis, under which disease she had laboured from the commencement of November 1827. The fits of coughing were very frequent and severe; expectoration partly purulent, and reported to have been tinged with blood for about a month, previous to her coming under my care. Breathing quick and oppressed, apparently easier when lying on the left side; skin warm; tongue white but moist; abdomen tumid; bowels reported to be in general costive; fœces slimy and fetid, usually of a dark colour, sometimes greenish. Pulse about 120; complexion pale; and she was considerably emaciated. She was nursed by Mrs ———, with whom she had lived from the evening of the day of her birth. Mrs ——— informed me that she was a fat flabby child till she was weaned, when about nine months old, and continued in tolerably good health till about two years ago, when she was affected with severe cough and difficult breathing. The medical gentlemen who attended her said that the disease was inflammation of the lungs. She was ill for three months, during which time leeches and blisters were applied repeatedly to her chest, &c. After she recovered from the above attack, she continued apparently in good health till she was seized with pertussis in the beginning of November last, as previously mentioned.

The usual treatment in cases of hooping-cough was adopted. The fits of coughing became less frequent, but the breathing continued much oppressed, and the expectoration purulent, but not mixed with blood. She became gradually weaker. The pulse rose to 130 and 140 in the minute; and for the few last days of her life her feet were œdematous; her urine in small quantity, with great thirst; her breathing was more and more oppressed, with considerable restlessness; and she died quite exhausted on Wednesday, the 6th February at twelve noon.*

The examination of the body took place on the following day at 1 P. M., and was conducted by my friend Mr Moodie.

Thorax.—On opening the thorax, there were strong adhesions found between the pleura pulmonalis and costalis on the right side; none on the left side. The left lung had *three lobes*, the right *two*. The lungs on both sides were very much diseased, but principally on the right side, which contained a great many small tubercles, some with purulent matter in the centre, and others assuming the appearance of small abscesses. The upper part of the left lung was in the same state as the right; the lower half more healthy. A considerable quantity of purulent matter exuded from the branches of the bronchiæ when cut. The bronchial glands were very much enlarged; and some of them consisted entirely of scrofulous cheesy matter. The pericardium was very much distended with serum. The heart was on the left side, but apparently nearer the centre of the body than usual. It was removed rather speedily, along with part of the blood-vessels attached, to be examined more minutely afterwards, so that the relative situation of the parts cannot be precisely stated. However, the part of the thoracic aorta, which was left in its situation, lay on the *right* side of the spine, but gradually reached the mesial line as it descended. On examination of the heart afterwards, nothing preternatural could be detected in its structure.

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* It may not be altogether irrelevant to notice, that the father and mother of this child, previous to their union, were reported to have been in a very close degree of relationship.

Abdomen.—The œsophagus passed through the diaphragm on the *right* side. The cardiac extremity of the stomach and spleen were also on the *right* side. The great lobe of the liver was on the *left* side; the smaller lobe towards the *right.* The commencement of the duodenum and the pylorus were on the *left* side, as likewise the cœcum, appendix vermiformis, &c. The sigmoid flexure of the colon occupied the usual place of the cœcum and ascending colon. It terminated as usual in the rectum, which made its way through the pelvis in the usual manner, excepting that it commenced on the *right* side.

The viscera of the abdomen appeared otherwise healthy. The kidneys, urinary bladder, and uterus, with its appendages, were in their natural situation, and without any morbid appearance.

The Brain.—The veins on the surface of the brain were very turgid; the substance of natural appearance. The ventricles contained a considerable quantity of limpid serum.

My friend Mr Moodie, who is one of Dr Knox's pupils, communicated to that gentleman a report of the dissection; and Dr Knox being desirous to have an opportunity of inspecting the body, requested me to obtain permission to have it re-opened, which was very readily granted; and accordingly Dr Knox, along with some of his pupils, Dr James Simpson, and myself, were present at the re-opening of the body the following day, the 8th February, at twelve noon. Dr Knox wrote a short account of the appearances which he observed, and requested my permission to send it to the editor of the London Medical Gazette for insertion in that periodical publication, which I with pleasure acceded to. The account of the abdominal viscera which I have read to the Society is almost verbatim in the words of Dr Knox, whose accuracy and indefatigable zeal in anatomical pursuits are well known to the members of this Society; and it affords me great satisfaction, that the deviation from the natural situation of the viscera which occurred in this case was witnessed by so able an anatomist.

The following is a list of some of the works in which similar cases of transposition of the viscera are to be found, viz.—

Bonetus, Sepulchret. sive Anat. Pract. Tom. iii. lib. iv. sect. xi. observ. 7th, p. 549. ("Cadaver sicarii cujusdam, dicti Richardi Francœur, qui supplicio rotæ fuit addictus anno 1630, dissectum fuit," &c.) A complete case of transposition of the viscera.

Schenkius, Observ. Med. Rar. lib. iii. obs. ix. p. 389. ("Hepar in sinistris, et lien in dextris.")

Bartholinus, Hist. Anat. Rar. Tom. i. cent. ii. hist. xxix, p. 219. (" Lien in dextro, jecur in sinistro hypochond.," &c.) Dissection at Paris in 1650, by Mag. Pet. Reguier.

London Philosophical Transact. No. cvii. 1674. (Account by Dr Sampson of the dissection of the body of J. D. a minister in Yorkshire, whose bowels were completely transposed.)

Hist. de l'Acad. Roy. des Sciences, 1688. (" M. Mery a fait rapport à l'Academie d'une dissection faite par M. Morand a l'Hotel Royal des Invalides du corps d'un Soldat mort à l'âge de 72 ans. Il y trouva un deplacement général de toutes les parties contenuës dans la poitrine et dans le ventre tant des viscéres que des vaisseaux," &c.)

Haller, Element. Physiologiæ, Tom. vi. pp. 118, 391, 460. (General observations on transposition of viscera.)

London Philosophical Transact. Vol. lxxviii. for 1788, part 2d, p. 350. (Account by Dr Matthew Baillie, of a remarkable transposition of the viscera, observed in the dissection of the body of a man near 40 years of age.)

Dr Duncan's Med. Comment. Vol. xiii. 1778, p. 428. (Short abstract of the above dissection.)

London Medical Journ. 1789, p. 178. (Account by Dr M. Baillie of the same dissection, with some alterations and additions.)

London Medical and Physical Journ. Vol. xxxvii. No. 218, for April 1817, p. 346. (General transposition of the viscera, observed by M. Beclard, in a woman about 50 years of age, who died of a pulmonary affection.)

Edin. Med. and. Surg. Journ. Vol. xix. for 1823, p. 652. " (Cases of three soldiers, reported by M. Scoutetten in *Journ. Univers.* 1823, who had passed their twentieth year, of good constitutions, and who had enjoyed excellent health, till they were cut off suddenly by gastro-enteritic inflammation. The anatomical details present nothing remarkable, except the extreme precision with which the viscera of the opposite sides occupied the place of one another.")

Nouvelle Bibliotheque Medicale pour Decembre 1827, p. 478. (" M. Serres a fait-voir le 24 Novembre dernier, aux élevès qui disséquaient à la Pitié, un cas de transposition des viscères," &c.) This is also a complete case of transposition. M. Serres intends to inject the subject, and to preserve it.

Several other works are mentioned by Ploucquet and Voigtel, in which transposition of the viscera was found more or less complete. Vide Ploucquet, Literatura Medica Digesta, under *Viscus dislocatio, Intestinum dislocatio, Hepar dislocatio, Splen dislocatio,* &c. and Handbuch der Pathologischen Anatomie von Dr F. G. Voigtel. Halle, 1804. Zweiter Band, p. 314.

7, York Place, Edinburgh, 5th March 1828.

PART III.

MEDICAL INTELLIGENCE.

PHYSIOLOGY.

On Cutaneous Absorption. (*Arch. Gén. de Médecine. xi.* 75, *and Bib. Méd. Juillet* 1827.) The subject of cutaneous absorption, and of absorption generally, has been lately investigated by M. Collard de Martigny. He has succeeded in establishing completely the reality of absorption through the skin, and has thrown considerable doubts over the doctrine advanced a few years ago by Magendie and Fodera, and now so generally adopted, that substances are absorbed throughout the animal system by a process of simple mechanical imbibition. As his experiments on these two topics appear to us important and conclusive, we shall give a short account of them. It is well known that Seguin, Magendie and other anterior physiologists have maintained, that the skin, when not deprived of its cuticle, is incapable of absorbing any substances which do not irritate the cuticle, or alter it in one way or another. This, however, does not appear to be the general opinion; yet we are not aware that any very conclusive arguments have hitherto been brought forward to prove the existence of simple absorption through the cuticle. The vagueness of the foundation on which the general opinion rested led M. de Martigny to endeavour to establish it upon more solid facts; and he has made a great variety of experiments, which seem to put its accuracy beyond all question.

He first placed upon the palm of the hand an inverted glass funnel filled with water, and remarked an obvious diminution of the water in the tube of the funnel after an hour or even less. He then varied this experiment, using precautions to preserve the pressure of the hand on the funnel uniform, as well as to prevent the evaporation of the water, and he still observed a sensible diminution. The following very simple experiment gave equally pointed results: In two small vessels of the same form and size he placed equal quantities of water, one being placed at his side exposed on the surface to the air, while into the other he put his two hands for half an hour. He then removed them and dried them in a handkerchief, the weight of which was taken both before and after this operation. The water of the two vessels was likewise weighed, and the proper correction being made for what was removed by the handkerchief from the hands, he found that 178 grains had been lost by absorption. Similar experiments were made with milk and with beef-tea, and the results were analogous It appeared to him that the beef-tea was absorbed more rapidly, and milk more slowly than water. On subsequently resuming the investigation he employed a still simpler method. He placed a few drops merely of the fluid to be examined on the inside of the arm, covering them with a small watch-glass, and allowing the experiment to go on for eight or twelve hours and upwards. In this way he remarked that water and brandy were absorbed altogether, and wine, beef-tea, and milk almost entirely. On next extending his researches to oleaginous substances, he arrived at the singular conclusion, that cutaneous absorption is not exercised upon them at all. Our readers will perhaps remember that Bichat, Magendie, Legallois, and others, have found that oleaginous fluids, directly introduced into the blood, obstruct its passage through the capillaries, and excite inflammation there. They likewise appear to undergo a species of digestion before they enter the lacteals, when they are swallowed as food; and further, when placed in contact with the serous membranes, they are absorbed very slowly, and not till they undergo a species of saponification. In a state of purity, therefore, they cannot enter the blood without causing serious injury. In the alimentary canal this is prevented by their digestion; in the serous cavities by their saponification through means of the serum. On the skin, where there is neither gastric juice nor a serous fluid, it is prevented by their being incapable of passing the cuticle. M. de Martigny found that they did not lose an atom of weight, after being eleven hours in contact with it.

The author concludes this department of his subject by endeavouring to determine the circumstances which regulate absorption through the skin. He has discovered that it is most rapid on those parts of the body where exhalation is greatest. He has farther remarked, that, while exhalation is going on, there is no absorption; that absorption is diminished during exercise or by an increase of temperature; and that it is increased by a depression of temperature or by the evacuation of blood. In short, cutaneous absorption is influenced by every particular in the inverse ratio of cutaneous exhalation.

The other question which the author has endeavoured to investigate relates to the mechanism by which absorption is effected. The experimental inquiries of Magendie and Fodera are conceived by many physiologists to be conclusive of the fact, that absorption is carried on by a process of simple imbibition; in other words, that in reaching the absorbent vessels substances percolate the living exactly as they do the dead tissues. It has always appeared to us, however, that the experiments of Magendie were liable to important objections, and that even the more conclusive experiments of Fodera, while they prove that mechanical imbibition or filtration does sometimes occur in the living body as through dead membranes, do by no means also prove that this phenomena is not materially under the influence of the vital principle. The experimental reasoning of these two physiologists has been very ably criticised by M. de Martigny, and its fallacies exposed. To the experiment of Magendie, which consists in insulating a vein and applying an active poison to its surface, he very properly objects that the poison may have entered the blood not by filtration but through the *vasa vasorum.* He admits, however, the conclusiveness of the experiment of Fodera, in which he found that substances injected into the peritonæum might be soon afterwards discovered in the chest; but he insists that the existence of such an imbibition is no argument against the existence also of a special action of absorption; and such a special action has, we think, been proved by him to occur in the following phenomena. Into the stomach of animals killed the previous day he injected a weak solution of hydrocyanate of potass, and four minutes afterwards he observed that the outer surface of the stomach became of a uniform blue colour when tested with the sulphate of iron. He then repeated the same experiment on living animals with the following results. Twenty minutes after the injection of the hydrocyanate, when none of it had reached the outer surface of the stomach, he poured several drops of the sulphate of iron over it. It was not till five minutes afterwards that he remarked here and there a faint blue tint not uniformly diffused as in the former instance, but arranged in a fine network; gradually the tint deepened and extended itself, its reticulated structure became less distinct, and at length a uniform blue stain was formed, as in the dead animal. It is not easy to see to what the slow progress of the fluid through the living stomach is to be attributed, unless to the permeability of the tissues being modified by their vitality. He has also endeavoured to establish the doctrine of special absorption by the following experiment, in which an attempt is made to show that absorption is under the influence of the nervous principle. Having divided all the nerves which supply the hind leg of the rabbit, he tightened a ligature round the whole thigh, exclusive of the vein and artery, and introduced a solution of the extract of nux vomica into the lower part of the leg. He then found that the poison operated more slowly than when the nerves were left entire. In conclusion, therefore, from these and other familiar facts, the author argues that the new doctrine of absorption must be modified by a partial return to the old; and he adopts the opinion, that this function is carried on by a special and elective action of the capillaries.

Nature and Composition of the Sweat and Insensible Perspiration. [*Zeitschrift für Physiologie, II. ii.* 1827.]—This subject formed the prize question of the Heidelberg Medical Faculty in 1821; and the following are the principal results obtained by the experiments of the successful candidate, Dr Anselmino. The insensible perspiration was collected by confining the arm for some hours in a long glass jar, the mouth being secured to the arm by fine oil-cloth, and the whole kept cool by a stream of cold water. By this method about a table-spoonful was procured in five or six hours. It was neither acid nor alkaline. It gave a copious precipitate with lime water; emitted ammonia when it was evaporated to dryness, with a little sulphuric acid, and then decomposed with potass; and gave out a strong acetic odour when it was digested with litharge, evaporated to dryness, and then treated with dilute sulphuric acid. Hence it contains carbonate and acetate of ammonia. If the arm was allowed to touch the glass vessel, other salts were also found in the fluid condensed on its inside; but these salts belong properly to the sweat. The volatilizable part of the sweat is similarly composed, but it contains an excess of acetic acid. The residue of the distillation at 212° consists of chloride of potassium, acetate of potass with an excess of acid, a small quantity of chloride of sodium, phosphate of lime, a small quantity of alkaline sulphates, a trace of iron, a matter analogous to salivary matter, and another resembling osmazome. It does not contain lactic acid, the acid once supposed by Berzelius to be the lactic, being, as that author subsequently conceived, the acetic rendered less volatile by being united with animal matter. The existence of iron in the sweat has been since confirmed by Professor Gmelin, who was induced by Anselmino's researches to inquire into this particular. Anselmino's Essay likewise contains a few remarks on the nature of the sweat in diseased states of the system; but his experiments on this department have been hitherto confined to its state in rheumatism. The sweat of a person who had been long affected with rheumatic fever, and profuse perspiration, was of the same nature and composition as natural sweat. The critical sweat of a case of acute rheumatism contained albumen, and the product of the distillation was not acid, but alkaline. Next day, after the crisis had been accomplished, it did not contain albumen.

On the Effects of extirpating the Kidneys. [*Zeitschrift für Physiologie II. ii.* 264.]—It has been recently proved by MM. Prevost and Dumas, that the removal of both kidneys is not necessarily followed by the cessation of the secretion of the characteristic principles of the urine; that in particular urea may be detected by a careful analysis in the blood; and that consequently we may presume the urea of the urine is not elaborated by the kidneys from the blood, but is merely separated by them from it. These results are very strikingly confirmed by experiments which have been lately published by Professor Mayer of Rome, but which appear to have been performed about the same time with those of the Swiss physiologists. Mayer remarked, that when both kidneys were removed from guinea-pigs, the animals generally perished during the first day, and no remarkable appearance could be detected after death. But some of them lived two days and upwards, and in them, serum effused into the peritoneal sac, into the chest, and even into the brain, as well as the contents of the stomach and intestines, the testicles, epididymis, vas deferens, and vesiculæ seminales, the liver, the substance of the lungs, the whole muscles, and the blood in the heart and great vessels exhaled a decided urinous odour. These phenomena, which were remarked in several animals, clearly show that not only is the urine separated in the kidneys from the blood by a species of elective filtration, if we may use the term, but likewise, that, when this outlet is shut up, it may be separated by almost every secreting organ in the body. The author, Professor Mayer, anticipates the objection, that the urine found throughout the body might have been absorbed from what remained in the bladder at the time of the operation; and replies to it that the absorption which takes place in that organ, while its communication outwardly remains open, is far too trifling to account for the presence of so large a quantity of urinous fluid throughout the whole system. The symptoms caused by the extirpation of the kidneys, according to Mayer, are gradual diminution of the pulse and breathing, and either death in a few hours without any other particular symptom, or convulsive movements of the abdomen, accompanied with cries, as of pain, increased discharge and more fluid consistence of the feces, rigors, profuse secretion of tears which smell of urine, and death generally in the course of thirty hours.

CHEMISTRY.

Analysis of the Mineral Waters of Scotland, by Dr Thomas Thomson. [*Glasgow Medical Journal, February* 1828.]

I. *Airthrey*, near Stirling, a mineral water in a beautiful and rich district of country, and which, since 1821, when they were first particularly attended to, have been increasing rapidly in request, and in the number of visitors. Dr Thomson has analyzed no fewer than six springs, the strongest of which contains 892, and the weakest 277 grains of solid matter in the imperial gallon of 277.274 cubic inches. The contents per gallon of the several springs are as follows:—

	First.	Second.	Third.	Fourth.	Fifth.	Sixth.
Mur. Soda,	423.843	359.616	263.948	135.792	513.060	537.567
Mur. Lime,	411.511	329.566	185 655	122.280	253.349	282.769
Sulph. Lime,	50.578	18.341	29.776	9.798	28.134	26.084
Mur. Magn.	6.075	4.168	1.597	9.546	13.713	2.438
	892.047	711.685	480.976	277.416	808.256	848.858

II. *Dumblane*, about eight miles from Airthrey, in a hilly and rather barren, and much less interesting country, has been a good deal resorted to; but since the discovery of the Airthrey springs, the visitors at Dumblane have become fewer, the former place possessing stronger springs, greater attraction from surrounding scenery, and more facilities of access. Its contents are the same as those of the Airthrey springs, and amount to 546 grains per imperial gallon. The proportions are,

Mur. Soda,	320.961
Mur. Lime,	174.366
Sulph. Lime,	48.551
Mur. Mag.	2.405
	546.283

III. *Pitcaithly*, on the banks of the river Earne, two miles from Perth, in a rich agricultural valley, and long frequented by many visitors. The contents per gallon amount to 329.266 grains; consequently it is weaker than either Airthrey or Dumblane. According to Dr Murray, whose analysis Dr Thomson adopts, the proportions are,

Mur. Soda,	128.674
Mur. Lime,	187.150
Sulph. Lime,	8.641
Carbon. Lime,	4.801
	329.266

IV. *Innerleithen*, near the Tweed, in a picturesque country, has been of late a good deal frequented, although its inland situation is rather against its becoming a favourite watering place. Dr Thomson adopts the analysis of Dr Fyfe, but conjectures that, besides the solid ingredients which that gentleman detected, it must likewise contain enough of free carbonic acid to constitute it an acidulous spring, otherwise the proportion of carbonate of magnesia could not be kept in solution. Its solid contents per gallon are in one spring 173, and in another 291 grains; and the ingredients are,

	Strongest.	Weakest.
Mur. Soda,	150.712	101.787
Mur. Lime,	91.320	45.612
Carbon. Magn.	49.107	25.447
	291.139	172.846

Dr Thomson promises to continue his valuable researches, and justly complains, that while the analysis of mineral waters is so much prosecuted abroad, and watering places are so much frequented in this country, so little has been done in the way of chemical inquiry for determining the precise nature of our native springs.

MEDICINE, THERAPEUTICS, AND TOXICOLOGY.

On the period of the disease at which Measles begins to be infectious. (Rust's Magazin, Februar. 1827.)—Various opinions have been entertained regarding the period at which small-pox, measles, and other exanthematic diseases begin to be capable of propagating themselves. It is unnecessary for our present purpose to particularise these opinions and the foundation on which they rest. It is enough to mention that many believe measles in particular not to be infectious till the eruption has broken out. The following observation lately made by Dr Harnier of Pyrmont establishes satisfactorily the contrary. A London merchant set off from London for Pyrmont on the 30th June 1825, along with his wife and three children, leaving behind, under the charge of a nurse, a fourth child, who had been attacked the day before apparently with catarrh. The party arrived at Pyrmont on the 8th of July, and had not been long there before they heard from London that the illness of the child left behind had turned out to be the measles, the eruption having appeared on the 1st July. Meanwhile one of the children at Pyrmont, the play-fellow of the one left behind, had been taken ill on the 9th with what appeared to be a common catarrh, and the rest of the family were not kept apart from him till the 11th. On the 12th the eruption of measles broke out. On the 24th and 25th of the same month the two other children, and three little girls belonging to the house where the family lodged, likewise fell sick, and in due course of time all of them had the eruption of measles. The five little patients had not seen the second child after the 11th, *the day before the appearance of the eruption*, and the three girls of the Pyrmont family had even been sent out of the house altogether. There was not a case of measles at the time in Pyrmont, and in consequence of the strict seclusion of the different patients, the disease did not spread.

On the Treatment of Intermittent Fever by the application of Sulphate of Kina to the Skin. (*Revue Médicale, Septembre* 1827.)—Doubts have been justly entertained whether the skin is capable of absorbing substances applied to it, unless the cuticle is previously abraded, or the substance is thrust through the cuticle by friction. It is agreed on all hands, however, that the surface of the true skin, stripped of its protecting cuticle, absorbs with rapidity; and our readers are probably aware that advantage has been lately taken of this physiological fact, in order to introduce into the system medicines which cannot be administered in sufficient quantity internally. M. Lesieur of Paris seems to have been the first who suggested the method now alluded to, and it has been since applied by himself and others to the administration of various active remedies. The paper of which a short analysis follows, is descriptive of the mode of treating intermittent fever by sulphate of kina administered in this way; and certainly the success and advantages held out by the author, M. de Martin, are very flattering. He states, that when the sulphate of kina is applied in a convenient form to a freshly blistered surface, it never, even when used in small quantities, fails to check the disease at once. In a state of purity it is ineligible, as it very soon irritates and inflames the surface of the true skin which is exposed to it, exciting livid redness, causing the formation of a gelatinous pseudo-membrane, and even destroying here and there the vitality of the surface of the skin, so as to produce small superficial sloughs. But when it is mixed in a state of fine powder with cerate, no such effects are produced, and in a short time it all disappears. The advantages of introducing the remedy through this channel are, first, that it can be safely and efficaciously used when owing to irritation or congestion in the alimentary it is either unsafe to administer it internally, or useless, on account of its being rapidly discharged by vomiting or purging;—secondly, that a less dose acts with equal power; thirdly, that it acts more rapidly as the system is sooner brought under its influence;—and lastly, that it can easily be administered to those who are averse to take internal remedies, and more particularly to refractory children.—The circumstances connected with the action of kina through the skin illustrate well its mode of operation. M. de Martin remarks, that it cannot act by effecting a revulsion, or as a counter-irritant, because local irritation is not only not necessary to success, but even also prejudicial; for he has invariably found that the remedy was least certain in its effects, and that the largest doses were required when inflammation was excited. This is easily explained on the principle that it acts through absorption; for the gelatinous effusion which is thrown out in the cases in which inflammation is caused obstructs its access to the absorbing surface. In point of fact, a great part of the powder always disappears when inflammation is not produced. The following cases will serve as examples of the author's plan of treatment. *Case VI.* A stout man, thirty-five years of age, had been affected for three months with a quartan ague, which had been checked four times by various preparations of cinchona. In the fifth attack, M. de Martin found him at the commencement of the fit with a frequent, strong, hard pulse, intense heat, flushed countenance, frequent strong cough, mucous expectoration, short breathing, pain under the left nipple, and a dry, red tongue. On account of the pulmonic affection, sixteen ounces of blood were taken from the arm. The fit returned again three days afterwards. Next morning, a blister having been applied three days before, and the part continuing to present a fresh surface, six grains of sulphate of kina were mixed with the cerate with which it was dressed, and twenty-four hours afterwards the dressing was changed, and six grains more applied in the same manner. He had not any return of the fit, and from that time improved rapidly in general health. The blistered surface was not in the slightest degree irritated. *Case V.* A woman, twenty-three years of age, long liable to pulmonary catarrh, and affected for a year previous with wandering pains in the chest, oppression there, and dry cough, was attacked towards the end of November 1826 with quotidian ague, of which she was cured by the internal exhibition of sulphate of kina. Towards the close of December the disease returned, still in the quotidian form. During the fit the heat was intense, the pulse strong and frequent, the general oppression considerable, the breathing difficult, and interrupted by paroxysms of violent cough. She was bled to eight ounces during the second paroxysm. On the morning of the third day, four grains of sulphate of kina, mixed with cerate, were applied to an old blistered surface. The paroxysm never returned. The dressing was renewed every twenty-four hours for four days. The cough and difficulty in breathing continued after the ague had long ceased. The blistered part became somewhat red at first, and a thin pseudo-membrane was formed; but the irritation, slight from the beginning, gradually diminished, and at length disappeared before the fourth dressing. *Case II.* This case exemplifies the kind of local inflammation which is caused by the remedy when not mingled with unctuous substances, as well as the effect of the inflammation in preventing its being absorbed. A soldier had suffered in the course of five months repeated attacks of tertian intermittent, and came under M. de Martin's care after the third paroxysm of a relapse. On the morning of the day on which he expected another, six grains of powdered sulphate of kina were sprinkled on a freshly blistered surface. The paroxysm appeared soon afterwards. Next morning the blistered surface had a deep red colour, was covered with a gelatinous exudation, and likewise showed here and there little yellow spots, which consisted of particles of kina covering little depressions in the cutis vera. Six grains of sulphate of kina were again applied, however. But now the pain became so intense, and the surface so red, that it was necessary to remove the powder, and apply a poultice. On the third morning the blistered part was covered with a thick, livid, moist eschar, having red edges, from which serum was discharged. The paroxysm reappeared this day, and again on the third, fifth, seventh, and ninth days afterwards. During this period the eschar became first black and dry, then round the edge soft and yellowish; and this softening extended inwards in the way usual with superficial eschars of the skin. After the sixth paroxysm the sulphate of kina was given internally, with the effect of checking the disease at once.

On the use of Tannin as a remedy for Menorrhagia. (*Annali Universali de Medicina*, *Aprile* 1827.)—This substance has lately been employed, it is said, with very great success, in the treatment of uterine hemorrhage by an Italian physician, Dr Porta. He has used it for three years, and has never found it fail but twice. The general results he has been led to by his practical experience are, that tannin has a special action on the uterus when the hemorrhage is active, whether it arises from chronic or acute metritis, but that in the latter case the inflammation must be previously combated by repeated copious blood-letting; that it is not of any use in hemorrhage connected with organic disease of the uterus; and that its action is very rapid, while the quantity required to effect a cure is so small as to be easily borne on the stomach. He has published several of the cases which have led him to these conclusions. The following is an abstract of a few of them. *Case I.* A woman of middle age, and of a bilious irritable temperament, after recovering from a severe attack of miliary fever, was attacked during convalescence with considerable uterine hemorrhage. Conceiving at first that it was nothing more than a premature appearance of the catamenia, she for some time paid little attention to it; but after it had continued several weeks, and had greatly reduced her strength, she became alarmed, and consulted Dr Porta. He ordered her to take two grains of tannin every two hours, and in two days the hemorrhage ceased altogether. *Case II.* A woman, thirty-two years of age, of delicate habit, and nursing a child four months old, was seized with uterine hemorrhage, which, like the former patient, she at first imagined to be the catamenial discharge. As it continued, however, much longer than usual, and was rapidly enfeebling her, she had recourse to Dr Porta, who found that she had been twenty days ill, and that she was affected with lumbar pains, some swelling and tenderness of the hypogastrium and symptomatic fever. He directed her to take a pill containing three grains of tannin every third hour; and she took only eight of them when the hemorrhage entirely ceased. *Case III.* A woman of irritable sanguine temperament, and thirty-eight years of age, had been affected for three weeks with menorrhagia, when Dr Porta was called to visit her. He found that pressure on the hypogastrium excited pain there, shooting towards the loins, that the pulse was hurried, and that she was liable to sudden paroxysms of pyrexia. As there was not, however, any notable swelling in the hypogastrium, he ordered the tannin without any preliminary treatment, and the same quantity was given as in the last case. In the course of two days about two scruples were taken, upon which the hemorrhage was permanently checked. *Case IV.* A young woman of robust habit was attacked with profuse hemorrhage from the uterus, by which in the course of a few weeks she was reduced to a state of great exhaustion. Notwithstanding her state, and the large quantity of blood she had lost, the pulse was frequent and full, and the hypogastrium was tumid and tender. Dr Porta immediately ordered her to cease taking wine and tonic drinks, which had been given her up to that time, and directed three grains of tannin to be administered every third hour. Towards the close of the fourth day the hemorrhage ceased. The lumbar pains which usually accompany the disease continued a few days longer.

Case of Death caused by the external application of Opium. (*Journal de Chimie Médicale, Avril* 1827.)—The application of opium in considerable quantity to the skin when deprived of its cuticle is a practice by no means free from danger; and when it is resorted to for allaying irritation, it is necessary to watch the patient carefully. We are acquainted with an instance in which deep coma was induced by the application of an opium poultice to the scrotum previously deprived of the cuticle by a blister, and which would in all likelihood have proved fatal, had not one of the patient's friends called accidentally to see him and discovered the cause of his state of sopor. The following is an example of death originating in the same source, although the skin was not deprived of its cuticle. A soldier, thirty-two years of age, was attacked with phlegmonous erysipelas of the fore and outer part of the right leg, on account of which a linseed poultice, moistened with *fifteen drops* of laudanum, was ordered to be applied to the limb. Next morning he was found in a state of deep sopor, with the face pale, the eyelids tremulous and half open, the pupils contracted, the lips distorted, the muscles of the face affected with spasm, and those of the limbs with convulsions. The surgeon having remarked a strong odour of opium, as well as a yellow coloration of the bandage which enveloped the limb, he had it removed, and found not only the bandage but all the compresses soaked with laudanum, which was flowing copiously from the poultice. The attendant whose duty it was to make up the poultice had left it in charge to an hospital servant without telling him what quantity of laudanum was to be used; and the latter had ignorantly used towards an ounce. Antispasmodics, emetics, and sinapisms, were resorted to, but in vain. The convulsions increased, the pulse became more and more feeble, and the patient died. On inspection some red points were seen on the arachnoid, a strong opiate odour was exhaled from all parts, and the heart, stomach, and brain were in their healthy. In the blood-vessels no trace could be found of the poison which had been absorbed.

Treatment of Poisoned Wounds. (*Journal des Progrès des Sciences*, T. iii. 1827.) M. Antoine Vernière has very recently announced to the Academy of Sciences a new method of treating poisoned wounds, which is, to say the least of it, ingenious and philosophically deduced from the late discoveries in the physiology of absorption, and which, if it stands the test of future experiment, will render the treatment of external poisoning exceedingly simple. The reader who is acquainted with the progressive researches of M. Magendie on venous absorption, will remember that physiologist discovered that absorption is impeded or even entirely prevented by producing an artificial state of venous plethora by injecting tepid water into the veins. M. Vernière's method of cure consists in taking advantage of this discovery, establishing a general venous plethora, and then removing the plethora as nearly as possible from that part only where the poison has been introduced by opening a vein proceeding from the part. Thus, three grains of the alcoholic extract of nux vomica were thrust into the paw of a dog, and a tight ligature was without delay tied round the limb. Warm water was then very slowly injected into the jugular vein to as great an extent as the animal could safely bear, and the ligature was removed. Half-an-hour was then allowed to elapse, which in ordinary circumstances would have been much more than sufficient to enable the poison to act. The ligature was next replaced, but so as to obstruct the venous circulation only, just as in performing the operation of venesection; the principal vein of the limb was then opened immediately under the ligature, and the blood which flowed out was collected. The blood was then carefully washed away, a little more was allowed to issue, and the ligature was finally removed. None of the symptoms of poisoning with nux vomica ensued, and the slight signs of venous plethora which remained were removed by a bleeding from the jugular vein. The blood which had been withdrawn from the vein of the fore-leg, when cautiously injected into the vein of another dog, caused violent tetanus and almost instant death. The explanation given by M. Verière of the mode in which this operation effects the removal of the poison is the following. " The only blood which can become impregnated with the poison is that which flows through the open vein, for it is only that vein and its branches which are relieved from the artificial plethora, and so rendered capable of carrying on the function of absorption. On the other hand, by means of the ligature, the current of the blood taking place from the artery only towards the opening in the vein, the poison introduced into the vessels is forced to follow the course of the blood, and is consequently thrown out of the system with it. " *Absorption, so deadly in all other circumstances, is thus converted into the means of safety. It searches for the poison in the depths of the tissues, and transports it into the veins, to be thence expelled from the body.*" The general venous plethora is in another respect advantageous: It enables us to transmit without fear of syncope, a considerable quantity of blood through the poisoned tissue, so as to clear it of the remains of the poison by means, as it were, of an internal washing."

Transfusion of Blood.—(*London Medical and Physical Journal for February* 1827.)—It is not long since we took notice of some instances of profuse uterine hemorrhage, in which life appeared to have been saved by the transfusion of blood. The following addition to the list is perhaps as strong a proof as any yet published of the propriety of the practice in desperate circumstances. A lady, who had repeatedly suffered from uterine hemorrhagy after delivery, brought forth her tenth child, and was immediately attacked with profuse flooding. The hand was introduced without delay, in order to stimulate the womb to contract, in consequence of which the placenta was soon expelled. The flooding ceased; but the patient had already lost so much blood as to be in a very alarming state of collapse. Brandy, camphor, and ether were accordingly given, cold fomentations were applied to the abdomen, and the body was laid inclined with the head lowest. Three violent attacks of convulsions nevertheless ensued, and they were followed by an increase of the collapse, so that her life was despaired of. The extremities became cold, the breast and abdomen alone retained warmth, the breathing was slow and stertorous, the eyelids shut, the pupils insensible to light and dilated, the lips tremulous, the pulse imperceptible at the wrists or in the neck. Her physician, Dr Brown, then became convinced that transfusion afforded the only chance of yet saving her life; and about two hours and a quarter after her delivery, thirteen drachms of fresh-drawn blood were gently injected into the median vein of the arm. Five minutes afterwards, no change having occurred, the same quantity was injected again, upon which the pulse began to return in the radial artery, while the breathing became easier and the pupils less dilated. In ten minutes more the same quantity was a third time injected: The amelioration was then quite evident, the pulse was regular, 120 in a minute, and the power of deglutition returned, so that she could swallow some spoonfuls of brandy. A fourth quantity, however, was also injected, (in all 6½ ounces); and then the chest was dilated fully at each inspiration, and the patient recognized all her friends around her. A state of violent reaction succeeded, but it was subdued, and the lady was eventually restored to perfect health.

Cases of Poisoning with Belladonna. (Journal de Chimie Médicale, Decembre 1827.)—Two children ate a considerable quantity of the berries of the Atropa Belladonna. The elder of the two on going to school immediately afterwards, burst out into immoderate and incontrollable fits of laughter, and constantly caught at objects which he conceived were before him. Subsequently was added to these symptoms an incessant, incoherent babble, with continual agitation of the body, fixed eyes, dilated and insensible pupils. This was above three hours after the berries had been taken. The physician who then saw him for the first time administered an emetic, which brought away a large quantity of reddish matter containing fragments of the berries. After this tartar emetic was given, though not without great difficulty, as the lower jaw was spasmodically contracted. Notwithstanding repeated vomiting the symptoms went on increasing, the limbs were in constant agitation, the breathing high and croupy, the face red, and the incoherence constant. Purgatives were then administered, and afterwards vinegar. Four hours after the poison was swallowed the breathing had become stertorous, the skin cold, the face bloated, the pulse imperceptible. The child was then immersed in the warm-bath and bled from the jugular vein to the extent of six ounces, with marked advantage. The muscular agitation was now broken by intermissions of profound sleep, in which, however, the patient was not permitted to indulge for twelve hours more. Next morning (nineteen hours) calomel was administered and caused several evacuations. In the afternoon the pulse again became almost imperceptible, the skin cold and pallid; and the warm-bath was therefore repeated and followed up by sinapisms and assafœtida injections, in consequence of which the pulse and temperature were restored. Four hours afterwards these symptoms recurred, and were again successfully combated by the same means. An hour and a-half after that, or 29½ hours after the berries were eat, the whole symptoms were much abated, and the child was sensible. From this time the amendment was rapid. But it was not till the third day that he could distinguish objects. Till then he was entirely deprived of sight: even a lighted candle held close to the eyes did not seem to make the slightest impression on the retina. For some time afterwards he was in indifferent health, and had occasional fits of slight convulsions; but he ultimately got quite well.—The circumstances of the other case were precisely the same in every respect.

ACOUSTIC SURGERY.

On the effects of injections of the Eustachian Tube in the treatment for the removal of Congenital Deafness. (*Revue Medicale, Avril* 1827.)—Not long ago a memoir was read before the Academy of Sciences by M. Deleau, in which high encomiums were bestowed on a plan which has occasionally been resorted for the removal of deafness, namely, the injection of water into the Eustachian tube. This operation has frequently been employed with success, in cases of casual deafness, succeeding catarrh, or, generally speaking, produced by some stoppage of the Eustachian tube. But M. Deleau had recourse to the same treatment in the instance of congenital deafness, and in several cases his efforts appeared to have been crowned with success,—so much so, indeed, that the Academy bestowed their approbation on the author's memoir. M. Itard, physician to the Deaf and Dumb Institution at Paris, who was engaged at the time in a series of experiments on the children of the institution at the request of the directors, had his attention naturally directed to the statements and alleged success of Deleau; and although he says he feared from physiological and pathological considerations that the high prospects held out by Deleau were illusory, he resolved to conduct a series of observations on his method on the large scale on which his opportunities enabled him to practise. Accordingly, in the course of eleven months he operated on no less than 120 of the pupils, employing the injection from three to ten times, and persevering from three to fifteen days, according to the sensibility of the ear, and the facility with which the injection was effected. The result of his labours may be summed up in a very few words. Unhappily this mode of treatment was of no essential benefit in a single instance. Sometimes the Eustachian passage was found to be quite free, sometimes on the other hand it was more or less obstructed. In the former case the injection always caused considerable pain; in the latter it caused very little till the passage was cleared and the admission of the fluid rendered easy: In short, M. Itard considers the production of pain in the internal ear by the injection a proof that the Eustachian tube is not obstructed. In most of the cases the pain ceased in an hour or more; in a considerable number slight headach ensued; in a few severer headach with fever for two or three days; and in seven inflammation and suppuration from the external ear. This last accident he did not use any means to prevent, because all the most remarkable instances of the cure of persons deaf and dumb from birth were accompanied by it. Although no material benefit was derived from the treatment in a single case, yet in a few a certain degree of amelioration was procured. In one class of patients, those whose deafness was the most complete, being eleven in number, the ear became sensible to sounds, but heard them confusedly, and the sensation was accompanied with pain. The hearing continued while the pain lasted, and in one case, in consequence apparently of regulated exercise of the ear, the hearing still continues although the pain is gradually ceasing. In all the rest the amelioration was lost after an interval varying from two to twenty days. In another class of patients, those who heard nearly best of all, amounting to twelve, a corresponding amelioration was effected, so that a number of vocal sounds which were previously heard confusedly, were heard distinctly; but in all of these cases the amendment was transitory. It existed longest in those who were subjected to regulated exercise; and M. Itard has had reason to conclude, that the amendment in all of them was owing as much to the exercise as to the operation. Nay, he states that, by means of regulated exercise alone, he has succeeded in accomplishing as material an amendment, as he witnessed in any of the patients on whom he performed the operation. On this subject he promises farther information, when his views have been subjected to more careful experiments.

He concludes his paper by an examination of the cases alleged to have been cured by M. Deleau by means of injections; and the result is, that he is led to deny altogether the reality of that gentleman's success. He notices in the first place some erroneous notions entertained by him as to the mode in which his process operates, and certain singular discrepancies between the account given by Deleau himself and that given in the report of the Academy on his paper; and he then shows that in reality the amendment accomplished has been very trifling, not greater indeed than took place in his own experiments. In every authentic case in which the hearing has been restored perfectly, the individual at once becomes capable of distinguishing and imitating sounds. Deleau's patients, on the contrary, have acquired that power but very imperfectly, very slowly, and only by means of regular exercise of the organs; and the most forward of his patients, after two years tuition, is not farther advanced than some of those whom M. Itard has improved by exercise without any preliminary operation. On the whole, therefore, he infers, that in the instance of those who are deaf and dumb from birth, injections of the Eustachian tube are of very little or no use.

OBSTETRICS.

Extra-uterine pregnancy, terminating in the discharge of the fœtus piece-meal by the anus.—(Journal des Progres des Sciences T. premier, 1827.*)*—A middle-aged woman, of a strong constitution, and well-constructed pelvis, and who had previously had three children without any particular accident occurring, became pregnant for the fourth time towards the close of October 1825. Nothing unusual occurred till the thirty-third week, when she began to complain of violent pains in the lower belly, confined chiefly to the left and anterior part of the pelvis near the groin, and quite different from the pains she had experienced in her former pregnancies. As these pains went on increasing, an accoucheur was consulted, who found the neck of the uterus situated high up, hard, and half-an-inch in length. About a fortnight afterwards she was persuaded to consult a *sorceress*, who lived three leagues and a half off, and who gave her a violent cathartic. Its operation was so violent that she was for some time in a dangerous state; but she recovered, and then it appeared that the pains had ceased, as well as the movements of the fœtus, and that the milk, which had begun to flow a short time before she took the cathartic, had receded. She became eventually so well as to be able to work; but she continued to purge frequently, and the discharges were fœtid and bloody. About three weeks and a half after she took the cathartic, she discharged a mass of flesh in a state of putrefaction and adhering to the bones of the leg of a fœtus; and its escape was followed by a copious putrid evacuation. For fifteen days after she discharged successively portions of bones, along with fluid fœtid evacuations; and in the course of that time was reduced to a very low state by hectic fever. At last there appeared in the left side of the abdomen a tumour, which soon suppurated, and bursting, discharged a large quantity of fœtid pus. The stools then became less frequent and she gradually recovered, so that in September 1826, she was in excellent health, though feeble. The neck of the uterus was in its usual condition, and the abdomen flaccid and not tender to the touch.

Case of Double Uterus, with Double Impregnation. (*Rust's Magazine, Vol. xx.* 568.)—An extraordinary case of this description has been related by Dr Geiss, a physician of Traffurth near Erfurt. The woman had been in labour for two days, when he was requested to attend her. He found her a stout, healthy female, of middle stature. The labour-pains were confined to the right side. On the right side, the womb reached as high as the thorax; on the left it did not extend above the navel, and inclined forwards and laterally. The external parts and os uteri were properly constructed, and the shoulder of the child could be felt behind the membranes. The operation of turning was performed, and a healthy female infant delivered, upon which the right side of the abdomen sunk in while the left continued prominent. In an hour the labour-pains recurred, and on examining the state of the parts, Dr Geiss found, that after passing the os uteri, the finger came in contact with a membrane distended by fluid, and protruding through an annular opening towards the left side, while the navel-string of the child already delivered, extended upwards into a cavity like the uterus in ordinary circumstances. On farther examination, he found the belly of a second child presenting at the accessory orifice, and therefore performed the operation of turning, and brought into the world a stout still-born male child, which was soon recuscitated. As no placenta followed for some time, he proceeded to assist the separation of them, and had then an opportunity of completely satisfying himself, that his patient had a double uterus. The right placenta came away first, and the right womb contracted vigorously; the left placenta followed, but the uterus of that side contracted slowly, and the woman therefore lost a good deal of blood. Two months afterwards the mother and both children were in good health. Two years before, she had brought forth a single child, and the labour was tedious, and required manual aid to accomplish.

Medical Statistics.

On the Relation which the Mean Stature and general perfection of the body bear to rank and mode of life. [*Annales des Sciences Naturelles, Juin* 1827.] M. Villermé has lately ascertained some curious facts on this subject, by consulting the returns made from the different districts or *arrondissements* of Paris and the surrounding country, of the young men fit for military service. It appears, in the first place, that of 11,735 persons examined for the department of the Seine, between 1816 and 1823 inclusive, no more than 5,825, or 49.7 per cent. were found fit for service; 50.3 per cent. were found unfit; and of these 8.7 were rejected on account of deformities, 12.6 because they were under the military stature, and 29 on account of disease and infirmities. The proportion rejected differed very considerably in the different districts of the city, being in some only 46.6, 45.7, and even 42.8 per cent., and in others so high as 52.3 per cent. The greatest number of rejections occurred almost invariably in the districts inhabited chiefly by the lower orders. A similar difference was remarked in the average stature of the young men who were examined. The average stature of those found fit for service was 1.683 metre, or 5 feet 6 and 3-10th inches English for the city of Paris, and 1.675 metre, or 5 feet 5 and 9-10th inches English for the adjoining country, forming the department of the Seine. The average stature of the recruits from the city differed in the different districts; and the extremes were 1.69 and 1.677 metre, or 5 feet 6 and 6-10th inches, and 5 feet 6 and 7-100th inches English, being a difference of half an inch. The higher averages were invariably procured from the districts abounding in families of the middle rank of life.

M. Villermé adds, that corresponding results were obtained in 1812 and 1813, in reply to queries transmitted to the prefects of the whole departments of France. The following is a very good example of the effect of sterility of the soil and poor living on the general developement and perfection of the frame. There are two cantons in the department of the Upper Loire, distant from one another about twelve miles, one of which is rich in soil, abounds in corn-fields and fine pastures, and is well clothed with healthy wood, while the other has a light soil, yields poor crops, and is sparingly clothed with stunted plantings. In the former, among the conscripts examined in 1806-7-8, only twenty-six per cent. were rejected as unfit for service; in the latter fifty-eight.

Table of the Diseases which cause death at various periods of Infancy. (*Roberton on the Mortality of Children.*)—This table is of so much importance, and of so rare a kind, that we have thought it worthy of receiving all the publicity possible.

Diseases.	Under 1 Month.	Between 1 and 2.	2 and 3.	3 and 6.	6 and 9.	9 and 12.	1 and 2 Years.	2 and 3.	3 and 5.	5 and 10.	Total.
Measles, -	—	1	1	5	8	25	117	72	50	20	299
Scarlet fever, -	—	—	—	—	1	—	—	1	4	2	8
Small-pox, -	—	1	2	5	25	17	49	30	44	14	187
Quinsey, -	—	—	—	—	—	—	—	—	—	1	1
Erysipelas, -	—	—	—	1	—	—	—	—	—	—	1
Swine-pox, -	—	—	—	—	—	—	—	—	1	—	1
Chin-cough, -	—	2	3	17	17	16	48	24	17	6	150
Croup, -	—	—	—	3	2	3	9	10	12	2	41
Inflammation of the lungs,	—	1	3	14	36	17	42	12	21	9	155
Do. of the bowels, -	4	2	—	4	8	3	7	6	1	3	38
Do. of the brain, -	—	—	—	1	—	1	—	—	—	1	3
Inflammatory fever, -	—	—	—	—	—	—	1	—	—	2	3
Inflammation of the liver,	—	—	—	—	—	—	2	—	—	—	2
Do. of the kidneys, -	—	—	—	—	—	—	1	—	—	—	1
Ulcerated throat, -	—	—	—	—	—	—	—	—	—	1	
Mortification, -	—	—	—	—	—	—	—	—	—	1	1
Water in the Brain, -	1	—	1	14	16	8	39	18	10	18	125
Convulsions, -	121	85	42	49	14	9	9	1	1	1	332
Fits, - -	—	2	—	4	2	1	1	4	4	—	18
Asthma, -	—	—	—	—	—	—	—	—	1	1	2
Water in the Chest, -	—	—	1	—	—	—	—	—	1	—	2
Disorder of the nerves,	—	—	—	—	1	—	—	—	—	—	1
Dropsy, -	—	—	—	—	—	—	—	—	—	3	3
Brain fever, -	—	—	—	—	—	—	1	1	—	5	7
Continued do. -	1	—	—	—	1	1	2	3	1	—	9
Typhus do. -	—	—	—	—	—	—	1	1	—	6	8
Worm do. -	—	—	—	—	—	—	2	7	1	1	11
Tooth do. and teething,	—	—	—	12	34	47	78	9	1	—	181
Remittent do. -	—	—	—	—	—	—	—	1	—	—	1
Rheumatic do. -	—	—	—	—	—	—	—	—	—	1	1
Putrid do. -	—	—	—	—	—	—	1	1	—	—	2
Cholera Morbus, -	1	1	2	6	1	4	4	1	—	1	21
Bowel complaints, -	5	11	7	17	15	12	10	4	2	2	85
Black thrush, -	—	—	—	2	—	1	1	—	—	—	4
Bilious complaints, -	—	—	—	1	1	—	—	—	—	—	2
Jaundice, -	—	—	1	—	—	—	—	—	—	—	1
Violent vomiting, -	—	—	—	—	—	—	1	—	—	—	1
Stoppage in the bowels,	1	—	—	1	—	1	—	—	1	—	4
Infantile decline, -	7	10	9	39	34	22	66	—	20	23	260
Consumption, -	—	—	—	—	—	—	—	—	6	12	18
Decline after measles,	—	—	—	—	—	—	3	1	1	—	5
Stricture of the bowels,	—	—	—	2	—	—	1	—	1	—	4
Defect in the internal organization, -	—	—	—	—	—	—	—	1	—	—	1
Inflammation of the head,	—	—	2	—	—	—	—	1	—	1	4
Tumour on the hip,	—	—	—	—	1	—	—	—	—	—	1
Inflammation in the groin,	—	—	—	1	—	—	—	—	—	—	1
Inflammation in the neck,	—	—	—	1	—	—	—	—	—	—	1
White swelling, -	—	—	—	—	1	—	—	—	—	—	1
Accidents, -	1	—	—	1	—	1	4	6	7	13	33
Unknown and lingering complaints, -	4	—	—	1	—	4	1	1	2	1	14
	146	116	74	201	218	193	501	246	210	151	2056

INDEX.

Page

Abercrombie, Dr, his researches on the diseases of the brain and spinal chord, . . 331
Absorption, cutaneous, on, . 444
Ague, sulphate of kina applied to the skin for, . . 448
Amenorrhœa, MM. Moulin and Guibert on its treatment, 218
Amputation at the hip-joint, Dr Mott's successful case of, 223
Amussat on the urethra, . 150
Anatomy, morbid, Dr Hooper and Dr Monro on that of the human brain, . . . 205
Annesley, Mr, his work on the diseases of India, . . 419
Arteries, Mr Syme's case of obstruction from an internal cause, 291
Asphyxia, Dr Kay's experiments on, . . . 37
Black, Dr, his case of hypertrophy of the heart, . . 303
Blood, Dr Davie on the specific gravity of its fibrin, . 244
Blood, on the stains caused by, on steel instruments, . 216
Blood, on its transfusion, 451
Bone, depressed, removal of, curing epilepsy, . . 219
Borland, Dr, his memoir of Dr Jackson, . . 110
Brain, Dr Hooper and Dr Monro on the morbid anatomy of, 205
—— Dr Robert Evans' case of tubercular tumour of, . 295
—— Dr Abercrombie's researches on the pathology of, . 331
—— Mr Davies Gilbert's case of disease of, . . 438
—— water of the, Dr Oppenheim on puncture in, . 358
Bronchotomy, Dr Cullen on the causes of its fatal termination, 75
Bryonine, on its physiological effects, 215
Calculus, case of very large one, 221
Calcutta, Transactions of the Medical and Physical Society of, 170
Campbell, Dr William, his case of laceration of the uterus and vagina, . . . 320
Carbonic acid, Dr Davy on that of the blood, . . 253
————— gas, on its narcotic properties, . . 215

Page

Caries, Dr Inglis Nicol on the treatment of, . . 1
Carotid ligature of, fatal by laceration, . . . 220
Catheter, female, Dr Montgomery on an improved form of, 325
Chicken-pox, &c., Dr Mohl on, 178
Cholera, fatal, Mr Tatham on its duration, . . 70
Christison, Dr, on poisoning, imputed and real, . . 18
Civiale on lithotrity, . 150
Combe, Dr, on poisonous mussels, 86
Cooper, Mr, his case of extra-uterine fœtation, . . 66
Corneitis and iritis, Mr J. Guthrie on, ... 315
Coup de soleil, Mr J. Mitchell on, 96
Craigie, Mr William, his successful case of tracheotomy, . 83
——— *Dr*, on the pathology of Whitloe, . . 255
Crichton, Mr, his observations and cases of lithotomy, . 225
Cullen, Dr, on the causes of the fatal termination of bronchotomy, 75
Davy, Dr John, his observations on various points relating to the chemical qualities of the blood, 244
Deafness, congenital, on curing it by injections into the Eustachian tube, . . . 452
Delivery, Dr R. Lee on, in presentation of the upper extremities, 239
Diffuse inflammation, Dr John Evans' case of, treated by incisions, . . . 281
Duct, lacrymal, Mr Teale on stricture of, . . 27
Elephantiasis, Arabian, Mr Hull's case of, . . . 31
Emphysema, case of spontaneous, 220
Epilepsy, cured by removing depressed bone, . . 219
Escharotic, muriate of zinc proposed as, ... 216
Evans, Dr John, his case of diffuse inflammation treated by incisions, 281
——— *Dr Robert*, his case of tubercular tumour of the brain, 275
Eustachian tube, injections into, for curing congenital deafness, 452
Eye, Mr Guthrie's lectures on the operative surgery of, . 119

Page

Fever, West Indian, (yellow,) Dr Wilson on, . . 189

Forbes, *Dr*, his translation of M. Laennec's work, . 406

Fœtation, Extra-uterine, Mr Cooper's case of, . . 66

Fungus Hæmatodes in the kidney, case of, . . 312

Gairdner, *Dr E.*, his case of fungus hæmatodes of the kidneys, ib.

Gangrene, Mr Macdermott's case of amputation during, . 289

Gilbert, *Davies*, *Mr*, his case of cerebral malformation, .

Guthrie, *Mr*, his lectures on the operative surgery of the eye, 119

——— *Mr James*, on corneitis, &c. . . . 315

Hanke, *Professor*, proposes muriate of zinc as an escharotic, . 216

Heart, Mr Vernon's case of hypertrophy of, . . 300

——— Dr Black's case of hypertrophy of, . . 303

Heat, on that given off during coagulation of the blood, . 250

Hemorrhage fatal, case of, from a varicose vein, . . 124

Hip-joint, Dr Mott's, successful case of amputation at the, - 223

Hooper, *Dr*, on the morbid anatomy of the brain, . 205

Hull, *Mr*, his case of Arabian elephantiasis, - - 31

Hydatids of uterus case of, 217

India, Mr Annesley's work on the diseases of, . . 419

Iritis and corneitis, Mr J. Guthrie on, . . 315

Jackson, *Dr*. memoir of, 110

Kay, *Dr J. P.* his experiments on asphyxia, . . 37

Kidney, case of fungus hæmatodes in, . . . 312

Kidneys, on the effects of their extirpation, . . 446

King, *Dr*. his case of unusually large calculus, . 221

Lac boxes, their manufacture and uses, . . 216

Laennec, Dr Forbes' translation of, 406

Larynx, Mr Liston's case of partial obliteration of, re-established, 118

Lee, *Dr Robert*, on delivery in cases of turning. . . 239

Leroy on lithotrity, . 150

Ligature of eighth pair of nerves, on the cause of death after, 214

Liston, *Mr*, his case of partial obliteration of the canal of the larynx, 118,—his case of lithotrity, 222, —his cases of lithotomy, 236

Page

Lithotomy, Mr Crichton's cases of, 225

———— Mr Liston's cases of, 236

Lithotrity, Leroy, Amussat, Civiale, and Tavernier on, . 150

————, Mr Liston's case of, 222

Leucorrhœa, MM. Moulin and Guibert on the treatment of, 218

Lombard, *Dr*, on the origin of tubercles . . 210

Luder's Dr, on small-pox after vaccination, . . 178

Macdermott, *Mr*, his case of amputation during gangrene, 289

Martin, *Colonel*, authentic account of his case, . . 221

Mayer, *Professor*, on death after ligature of the eighth pair, 214

Mayo, *Mr H.*, his Elements of Physiology, . . 141

Mercurial salivation, Dr Finlay on its treatment, . 218

Measles, the period of their infection, . . 447

Medico-legal cases, Mr D. Syme's report of, . . 410

Mineral Wells of Scotland, Dr Thomson's Analysis of, . 446

Mitchell, *Mr James*, on coup de soleil, . . 96

Mojon, *Signor*, his laws of physiology, . . 141

Mohl, *Dr*, on chicken-pox and varioloid eruptions, . 178

Monro, *Dr*, on the morbid anatomy of the brain, . 205

————, his notice of Colonel Martin's case, . 221

————, his illustrations of the pelvis, . . 366

Moncreiff, *Dr*, his case of transposition of the viscera, . 440

Montgomery, *Dr*, on an improved form of the female catheter, 325

Mortality, Mr Roberton's inquiries on that of children, 373—455

Mott, *Dr*, his case of amputation at the hip-joint, . 223

Muriate of zinc, its escharotic powers, . . 216

Murray, *Mr*, his case of diseased ovaries, . . 305

Mussel, Dr Combe on the poison of, 86

Nerves, eighth pair, on the cause of death after ligature of, . 214

Nicol, *Dr Inglis*, on the treatment of caries, . . 1

Opium, East Indian, its characters, 216

———, poisoning by, on artificial respiration, in, . . 220

———, death produced by its external application, . 450

Page
Oppenheim, *Dr*, on cases of hydrocephalus, . . 358
Ovaries, Mr Murray's case of organic disease in, . . 305
Pelvis, Dr Monro's illustrations of its anatomy, . - 366
Physiology, Mr Mayo's outlines of, 141
———, Signor Mojon's Laws of, 141
Plumbe, *Mr Samuel*, his work on diseases of the skin, . 385
Poisoning, Dr Christison on imputation of, . . 18
——— by belladona, cases of 451
Poisoned wounds, on their treatment, . . 450
Pregnancy Extra-uterine, case of, 453
Presentation of upper extremities, Dr Lee on delivery in, - 239
Quinine, sulphate, on large doses of it, . . . 218
Rayer, *M.*, his work on diseases of the skin, . . 385
Renton, *Dr Robert*, on the efficacy of spurred rye, . 322
Respiration, artificial, on its use in poisoning by opium, . 220
Roberton, *Mr*, his inquiries on the mortality of children, 373
Rye spurred, Dr Renton on its efficacy, . . 322
Salivation, mercurial, Dr Finlay on its treatment, . . 218
Salmon, *Mr*, on stricture of the rectum, . . 399
Senna, East Indian, its characters, 216
Skene, *Mr George*, his translation of Mojon's Laws of Physiology, 141
Skin, M. Rayer and Mr Plumbe on diseases of the, . 385
——— on absorption by, . 444
Small-pox, Dr Luders on its occurrence after vaccination, . 178
Stature, on the relation between, and mode of life, . . 454
Stricture, Mr Salmon on that of the rectum, . . 399

Page
Sulphate of quinine, large does of it, . . . 218
Sulphate of kina applied to the skin for ague, . . 448
Sweat and perspiration, on the nature and composition of, . 444
Syme, *Mr James*, his case of obstruction of the arteries from an internal cause, . . 291
Syme, *Mr David*, his report of medico-legal cases, . 410
Tannin used for menorrhagia, 449
Tatham, *Mr*, on the duration of fatal cholera, . . 70
Tavernier on lithotrity, . 150
Teale, *Mr*, on stricture of lacrymal duct, . . . 27
Transactions of Medical Society of Calcutta, Vol. ii. . . 170
Transfusion of blood, case of its effects, . . . 451
Transposition of the viscera, Dr Moncreiff's case of, . 440
Tracheotomy, Mr William Craigie's case of, . . . 83
Tubercles, Dr Lombard on the origin of, . . 210
Urinary concretion of unusual dimensions, case of, . 221
Uterus, case of hydatids of, 217
———, case of double, with double impregnation, . .
———, Dr William Campbell's case of laceration of, . . 320
Varicose vein, fatal hemorrhage from, . . . 224
Vernon, *Mr*, his case of hypertrophy of the heart, . 300
Wilson, *Dr John*, on yellow fever, 189
Whitloe, on its pathology and treatment, . . . 255
Wounds, poisoned, on their treatment, . . .
Zinc, muriate, of its escharotic powers, ...216

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